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ABSTRACT

The Worker Adjustment and Retraining Notification Act (WARN) was passed in 1987 to provide advance notice to workers whose jobs are to be eliminated because of plant closings and relocations. A study examined the extent to which advance notice eases the adjustment problems associated with permanent loss of jobs. Data from the Displaced Worker Supplement to the January 1988 Current Population Survey, which contain information on the type and timing of notice and on the duration of the initial spell of joblessness, were analyzed. Six primary findings emerged: (1) prior to the passage of WARN, few firms voluntarily provided written advance notice of 60 days or more; (2) previous research has overestimated the extent to which prenotification reduces postdisplacement joblessness; (3) written announcements of layoffs and plant closings increase the probability that displaced workers obtain new positions without intervening joblessness--however, there is no indication that formal notice reduces average joblessness for those failing to find other jobs; (4) formal advance notice provided more than 2 months prior to displacement may substantially raise earnings in the new job; (5) further research is needed before the effects of mandatory notice can be predicted confidently; and (6) the impact of prior notification varies widely over population subgroups, with married persons, household heads, and workers in high-unemployment areas benefiting most. (46 references) (KC)

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The Impact of Advance Notice Provisions on Postdisplacement Outcomes

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EXECUTIVE SUMMARY

The Impact of Advance Notice Provisions on Postdisplacement Outcomes

In the United States, plant closing legislation has been introduced in every congress since 1973. In 1985 a bill (H.R. 1616), which would have required companies terminating more than 50 employees (at a single plant) to provide 90 days advance notice, was narrowly defeated in the house. In 1987 the Senate passed a trade bill (S. 1420) which included provisions for a mandatory prenotification period of 30 Days. After vetoing the Omnibus trade bill explicitly because of its notice provisions, the Reagan administration allowed P.L. 100-379, the Worker Adjustment and Retraining Notification Act (WARN) -- which contained a virtually identical mandatory notice provision -- to become law when it was presented in separate legislation.

The passage of WARN has not ended debate over the efficacy of mandatory notice. Advocates claim that early warning eases the adjustment problems of displaced workers and enhances their ability to search for new employment. Prenotification may also allow for the implementation of on-site training and placement programs. In some case, it may even provide the time to implement corrective measures which may tend to mitigate layoffs. Opponents counter that advance notice leads to early departures of the most productive employees, worker sabotage, and general disruption of firm operations. By increasing the costs of terminating workers, the provisions may reduce hiring during periods of high demand. They may also reduce the ability of the firm to market its products and make it more difficult to sell the plant.

This study examines the extent to which advance notice eases the adjustment problems associated with permanent loss of jobs and improves on shortcomings in related previous work. A newly available data set, the Displaced Worker Supplement to the January 1988 Current Population Survey is used, which contains information on the type and timing of notice, and on the duration of the initial spell of joblessness. A careful and comprehensive examination of the impact of advance notice on postdisplacement wages is undertaken and a switching regression framework is employed to provide a preliminary investigation of endogeneity biases. Advance notice is also allowed to have a differential impact across population subgroups.

Six primary findings are highlighted. First, prior to the passage of the Worker Adjustment and Retraining Notification Act, relatively few firms voluntarily provided written advance notice of 60 days or more. Between 1983 and 1987, only 15 percent of dislocated individuals received any formal advance

notice and just over 5 percent were provided with written announcements at least 2 months before their jobs terminated. Specific verbal notice was also furnished relatively rarely.

Second, previous research has overestimated the extent to which prenotification reduces postdisplacement joblessness. This occurred because earlier data sets did not allow researchers to distinguish between formal and informal notice.

Third, reduced form regressions continue to show that written announcements of layoffs and plant closings increase the probability that displaced workers obtain into new positions without intervening joblessness. There is no indication, however, that formal notice reduces average nonemployment for those failing to do so.

Fourth, formal advance notice provided more than 2 months prior to displacement may substantially raise earnings in the new job but there is no corresponding evidence of favorable effects for informally notified workers or for those obtaining written warning shortly before job termination dates.

Fifth, the endogeneity of notice appears to be quite important, especially when considering postdisplacement wages. Unfortunately, its effects are only partially captured by the switching regression analysis framework employed in this study. Further research is therefore required before the effects of mandatory notice can be predicted with confidence.

Sixth, the impact of prior notification on postlayoff joblessness varies widely across population subgroups. Of particular importance, relatively large benefits are obtained by household heads, married persons, and displaced workers residing in local labor markets with high rates of unemployment. Joblessness is generally considered especially problematic for each of these groups.

A seventh, somewhat peripheral, result is that the determinants of postdisplacement nonemployment are estimated fairly robustly across a wide variety of regression models. This suggests that researchers have some flexibility in choosing the method which is most convenient for the problem in question. For example, the linear model, which is required for switching regressions, provides coefficient estimates which are highly consistent with those obtained using duration and other censored regression models.

With the passage of the Worker Adjustment and Retraining Notification Act, the United States has joined virtually all other industrialized countries in regulating plant shutdowns and mass layoffs. Given the short period of time since the law has taken effect, it is too early to assess the degree to which it has changed employer behavior or assisted displaced workers. The results of this study, however, suggest that provision of at least 2 months written advance notice is likely to lead to modest benefits for displaced workers. In particular, these persons are expected to be somewhat more likely to avoid joblessness and to have slightly higher reemployment earnings. Nonemployment durations are also substantially reduced for a number of groups for whom nonemployment is of special concern.

The benefits of advance notice must be weighed against any costs to employers. Unfortunately, reliable research on the latter is virtually nonexistent. Despite predictions that customers may disappear, access to credit markets will be impaired, productivity reduced, or absenteeism increased, the limited information which is available provides little evidence of *any* serious negative consequences for business.

On balance, the existing research, including that presented in this report, lends provisional support to the advance notice legislation passed in 1988. Beyond any economic consequences, workers favor mandatory early notification because they feel it to be fair, while the corporate sector expresses concern that it represents a first step towards more onerous government inteference. Debate over the proper role of the government role is likely to continue. Nontheless, employment security appears to have joined child labor, occupational safety, and equal opportunity as an area where federal regulation has become an accepted fact.

The report concludes by suggesting a number of directions for subsequent research including: more sophisticated attempts to control for the endogeneity of advance notice, explicit modeling of the process by which workers become informed of future displacements, greater attention to demographic group variations in the effects of prior notification, and further investigation into the importance of the timing of the receipt of advance notice. A number of improvements in the *Displaced Worker Supplements* data are also recommended.

Chapter 1:

Introduction

Dynamic economies engage in a process which Schumpeter has described as "creative destruction". New enterprises are continually forming while old ones disappear. Within existing companies, production processes change, organizations evolve, and labor requirements adapt to an ever shifting market place. In the last two decades, increased international competition has caused additional pressure and, now more than ever, long-term success requires the ability to respond to continual flux and to strive to reach an ever moving target. Substantial changes in labor force patterns and population demographics have further modified work practices and norms of corporate behavior.

One inevitable consequence of creative destruction is that jobs disappear and, in some cases, workers are displaced from companies. Rapid advances in manufacturing, materials, transportation, and telecommunication technologies have led to accelerating rates of structural change, while increased international competition has added pressure for down sizing and greater use of temporary and subcontracted labor. These factors, combined with the secular shift of employment from manufacturing to services, have led to a rise in the number of economically dislocated workers.

At the same time that the risk of displacement has been increasing, the ability of workers to find comparable new jobs has become more uncertain. The economy was buffeted by back to back recessions of a severity unparalleled since the great depression in the early eighties and the subsequent recovery, which now has lasted 6 years, has been fairly strong but quite uneven. Thus while unemployment rates are low on both coasts, they remain at recession levels throughout much of the interior of the country. Although total employment has increased and unemployment rates are about 2 percentage points below the 1980 level, manufacturing employment continues to decline, even in economically healthy states such as Massachusetts.

Uneven economic growth, extended joblessness, declining manufacturing employment, and a fear that well-paying jobs may be disappearing have all increased concerns over the problems facing displaced workers. In response, the Trade Assistance Adjustment Act (TAA) of 1974, and Title III of the 1982 Job Training Partnership Act (JTPA) provide various forms of support for dislocated individuals including supplemental unemployment benefits, training, and reemployment assistance. Provisions in the recently legislated omnibus trade bill, will lead to an almost threefold increase in federally funded training efforts targeted at displaced workers — to almost 1 billion dollars annually. Hand-in-hand with this increased budgetary support, the United States has recently witnessed a lively policy debate over the appropriate government role in regulating employee dismissals.

Most industrialized countries have legislation regulating plant shutdowns and mass layoffs. Typically, employers are required to negotiate with employees (or their unions) and the government in an effort to avert the terminations. Failing this, they are usually required to provide some combination of severance pay, advance notice, and continuation of benefits. The period of required notice is around 1 month in Belgium, Denmark, Germany, Greece, Ireland, Italy, and the Netherlands, and is 2 months or more in other Western European countries (France, Luxembourg, Sweden, and Britain) and in most parts of Canada.[1] Small firms and employers laying off few workers are typically exempted from notification requirements. Some countries, such as Sweden, coordinate labor and industrial policies through a single agency which attempts to weigh the tradeoffs between worker dismissals and firm needs.[2] Canada's Industrial Adjustment Service operates as a catalyst in developing adjustment strategies to redeploy workers both within and outside the firm.[3]

In the United States, plant closing legislation has been introduced in every congress since 1973. In 1985 a bill (H.R. 1616), which would have required companies terminating more than 50 employees (at a single plant) to provide 90 days advance notice, was narrowly defeated in the house. In 1987 the Senate passed a trade bill (S. 1420) which included provisions for a mandatory prenotification period of 60 days. After vetoing the Ombnibus trade bill explicitly because of its notice provisions, the Reagan administration allowed P.L. 100-379, the Worker Adjustment and Retraining Notification Act (WARN) — which contained a virtually identical mandatory notice provision — to become law when it was presented in separate legislation. This occurred because of extremely widespread public sentiment in favor of mandated prior notification (polls indicate support by more than 80 percent of the public) and the desire to avoid making this a campaign issue in a presidential election year.[4]

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[1] Greater detail on international advance notice requirement is provided in Ehrenberg & Jakubson (1988, pp. 2-4). According to Hanami (1982), the period of required advance notice is 30 days in Japan.

[2] Hooks (1984) discusses Swedish policies in some detail.

[3] The operation of the IAS is discussed in U.S. Secretary of Labor (1986).

[4] See Addison and Portugal (1989) for a fuller discussion of the events leading up to the passage of WARN.

The passage of the first national legislation requiring advance notice came after more than 40 proposed laws at the federal level since 1979 and 125 bills in 30 states between 1975 and 1983 (Ehrenberg and Jakubson, 1988). Prior to the federal regulation, 3 states (Maine, Wisconsin, and Hawaii) had implemented mandatory advance notice provisions, with 3 others (Massachusetts, Maryland, and Michigan) implementing voluntary programs encouraging companies to provide early warning or continue employee benefits.

WARN contains numerous exemptions. Employers are required to provide 60 days advance notice of layoffs or plant closings except in the following cases: 1) the company employs fewer than 100 persons or is laying off less than 50 workers; 2) companies are terminating less than a third of their workforce, unless the total number of layoffs exceeds 500; 3) closings due to "unforeseeable" business developments, strikes, or lockouts; 4) the faltering company is actively seeking new capital or business; 5) the plant closing is due to sale of a business or consolidation within a local area; 6) the workers are offered new positions. These exclusions substantially reduce the number of workers who will be prenotified. Firms failing to supply adequate advance notice are required to pay workers for the period over which notification should have been provided and also are subject to $500 per day fines for failure to notify local governments. Employees or local governments are required to bring suit in federal district courts to insure compliance and these courts determine whether an exemption applies.

The passage of WARN has not ended debate over the efficacy of mandatory notice. Advocates claim that early warning eases the adjustment problems of displaced workers and enhances their ability to search for new employment.

Prenotification may also allow for the implementation of on-site training and placement programs. In some cases, it may even provide the time to implement corrective measures which layoffs. Opponents counter that advance notice leads to early departures of the most productive employees, worker sabotage, and general disruption of firm operations. By increasing the costs of terminating workers, the provisions may reduce hiring during periods of high demand. They may also reduce the ability of the firm to market its products and make it more difficult to sell the plant.

This debate has taken place largely in the absence of information about either the benefits or costs of mandatory notice. The situation has recently improved, however, with the release of several studies using nationally representative data from special Displaced Workers Supplements (DWS) to the January 1984 and January 1986 Current Population Surveys (e.g. Addison & Portugal 1987a, 1987b; Kletzer 1987; Podgursky & Swaim 1987a, 1987b; Ehrenberg & Jakubson 1988).[5]

The main focus of this work has been to examine whether early notice reduces postdisplacement joblessness. The general conclusion is that the average time out-of-work is around 1 month lower for notified workers than for their non-notified peers. This occurs mostly because they are more likely to move directly into new positions and avoid nonemployment altogether. There is little evidence of falling joblessness for persons who are unable to obtain immediate reemployment.[6] There has been less research studying the effects of advance warning on postdisplacement wages. The little that has been done, provides no indication that subsequent earnings are raised by prior notification.

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[5] Also see Folbre, Leighton, & Roderick (1984) for an earlier investigation using enterprise level data for Maine. There is still little hard evidence of the costs of advance notice to businesses. The data that is available suggests few serious negative consequences. Productivity often rises, rather than falling, during the notice period and notification sometimes results in actions which save rather than destroy the plant (Berenbeim, 1986; Sutton, 1987). Although some employees quit after receiving notice, the manpower losses are generally minor and do not prevent efficient operations of the affected enterprises (Weber & Taylor, 1963). Lazear (1987) presents evidence that advance notice is associated with small and statistically insignificant reductions in the employment/population ratio. These findings are qualified, however, by his use of annual aggregate data (for 23 countries) which includes relatively few changes in notification regimes.

Unfortunately, the usefulness of earlier research is lessened because of limitations inherent in the data sources and methodological approaches used. Three data shortcomings are particularly troublesome. First, because the 1984 and 1986 DWS provide no information on either the duration or type of notice, investigators were unable to distinguish between written notification, verbal announcements, and expectations of job loss in the absence of any type of notice. Second, they could not differentiate the impacts of short versus lengthy prenotification. Third, data on joblessness is limited to total weeks out-of-work between the time of displacement and the survey date, whether this transpires in a single spell or in multiple occurrences punctuated by short periods of employment. This is problematic for econometric duration models which require continuous spell information. For policy purposes, we are also often interested in the initial period of joblessness.

Previous studies have also paid relatively little attention to the potential endogeneity of advance notice and have focused on average impacts rather than on the potentially large variations across worker, job, and geographic characteristics. Endogeneity bias could cause the effects of prior notification to be either under or overestimated. For example, if firms more frequently notify workers when local labor markets are depressed than when they are healthy, early warning is likely to be associated with only small reductions (or even increases) in joblessness. This occurs because new employment is more difficult to obtain when area unemployment rates are elevated, rather than because prenotification delays job finding. Conversely, if individuals who are especially averse to unemployment are both more likely to work for employers providing notice and also obtain relatively rapid reemployment following terminations (because they are less selective about accepting new positions), advance notice will be negatively correlated with postdisplacement joblessness.[7] Even if the average impact of early warning is fairly small, large benefits might accrue to population subgroups experiencing reemployment difficulties or for those whose joblessness raises particular concerns.

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[6] Because the DWS does not distinguish between unemployment and nonparticipation in the labor force, the terms joblessness or nonemployment (rather than unemployment) will be used throughout this report.

Finally, the relative scarcity of analysis on the effects of advance notice for postdisplacement wages is distressing. Related research examining the adjustment problems of displaced workers suggests that wage effects are important and lasting. For example, Ruhm (1989a) shows that although postlayoff joblessness is largely transitory, a substantial portion of the associated earnings changes persist for long time periods.[8] Policies enabling job losers to become reemployed at higher wages are likely to yield lasting benefits and it is important to discover whether prenotification has this desirable effect.

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[7] Ehrenberg & Jakubson (1988) devote considerable attention to the endogeneity problem but are unable to resolve it with any satisfaction. In discussing their effort, they conclude "we are skeptical of our ability to use the estimates obtained here to control for the endogeneity of advance notice in the duration of nonemployment and postdisplacement wage equations" (pp. 45).

This study improves on each of the above shortcomings. First, a newly available data set (the 1988 DWS) is utilized which contains information on the type and timing of notice, and on the duration of the initial spell of joblessness. Second, a careful and comprehensive examination of the impact of advance notice on postdisplacement wages is undertaken. Third, a switching regression framework is employed to provide a preliminary investigation of endogeneity biases. Finally, a full set of interaction terms are included in the switching regression model to allow for variation in the impact of notice across population subgroups.

Six primary findings are highlighted in the chapters which follow. First, prior to the passage of the Worker Adjustment and Retraining Notification Act, relatively few firms voluntarily provided written advance notice of 60 days or more. Between 1983 and 1987, only 15 percent of dislocated individuals received any formal advance notice and just over 5 percent were provided with written announcements at least 2 months before their jobs terminated. The evidence further suggests that specific verbal notice was also furnished relatively rarely.

Second, previous research has overestimated the extent to which prenotification reduces postdisplacement joblessness. This occurred because earlier data sets did not allow researchers to distinguish between formal and informal notice.

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[8] Also see Ruhm (1989b) for evidence on transitory joblessness and Jakobson (1984), Podgursky & Swaim (1987b), and Ruhm (1987) for indications of persistent wage changes.

Third, reduced form regressions continue to show that written announcements of layoffs and plant closings increase the probability that displaced workers obtain into new positions without intervening joblessness. There is no indication, however, that formal notice reduces average nonemployment for those failing to do so.

Fourth, formal advance notice provided more than 2 months prior displacement may substantially raise earnings in the new job but there is no corresponding evidence of favorable effects for informally notified workers or for those obtaining written warning shortly before job termination dates.

Fifth, the endogeneity of notice appears to be quite important, especially when considering postdisplacement wages. Unfortunately, its effects are only partially captured by the switching regression framework employed in this study. Further research is therefore required before the effects of mandatory notice can be predicted with confidence.

Sixth, the impact of prior notification on postlayoff joblessness varies widely across population subgroups. Of particular importance, relatively large benefits are obtained by household heads, married persons, and displaced workers residing in local labor markets with high rates of unemployment. Joblessness is generally considered especially problematic for each of these groups.

A seventh, somewhat peripheral, result is that the determinants of postdisplacement nonemployment are estimated fairly robustly across a wide variety of regression models. This suggests that researchers have some flexibility in choosing the method which is most convenient for the problem in question. For example, the linear model, which is required for switching regressions, provides coefficient estimates which are highly consistent with those obtained using duration and other censored regression models.

The remainder of this report is structured as follows. Chapter 2 provides descriptive information on the frequency with which advance notice is received and on the relationship between prior notice and postdisplacement joblessness, reemployment earnings, and survey date employment probabilities. In addition to aggregate effects, we examine whether there are variations across population subgroups. Chapter 3 follows with a probit regression analysis of the determinants of advance notice. Chapters 4 and 5 then examine the effects of written and unwritten advance notice on postdisplacement joblessness and wage changes. This analysis is performed using a reduced form regression models which implicitly assumes that the prenotification variables are exogenous. Chapter 6 follows by employing a switching regression framework to allow for the endogeneity of advance notice. Chapter 7 concludes the report by summarizing the results and discussing implications for policy.

Chapter 2:

Advance Notice - Descriptive Information

This chapter provides descriptive information on the receipt of advance notice, the characteristics of workers obtaining it, and on the association between notification and postdisplacement joblessness, wage changes, and reemployment probabilities. This general description precedes the regression analysis of chapters 3 through 6, where a wide variety of covariates are controlled for. It provides an initial indication of the determinants and effects of prenotification. We begin this chapter by discussing the data set used throughout this report.

2.1 Data

This report uses data from the _Displaced Worker Supplement_ to the January 1988 _Current Population Survey_. The 1988 _DWS_ contains retrospective information on previous job histories and labor market status for a nationally representative sample of workers suffering permanent job loss between January 1983 and January 1988. Additional information on current labor force status is available from the regular monthly _CPS_. The sample analyzed includes workers between the ages of 25 and 60 (at the survey date) who lost jobs as the result of a business failure, plant closure or relocation, or a layoff resulting from slack work or position or shift abolished. Persons terminating jobs in agriculture, construction, or the armed forces are excluded, as are previously self-employed individuals and those displaced in the month of the survey.

The 1988 _DWS_ includes 3 questions pertaining to advance notice. The first inquires whether the worker did "expect a layoff or had received advance notice of a layoff or plant or business closing?" This inquiry, which contains no information on either the type or timing of notice, was also incorporated in the 1984 and 1986 DWS supplements and provided the only information on early notification available to previous researchers. The 2 questions added to the 1988 DWS ask if the respondent had "been given written advance notice that the business would be closed or that he/she would be laid off" and, if so, "how long before he/she was to be laid off did he/she receive that notice?" Responses for the last question were categorized into the ranges: less than 1, 1 to 2, and greater than 2 months.

In addition to data obtained directly from the DWS, information was used from other sources. Variables indicating state, industry, and occupation unionization rates, and a dummy variable for respondents residing in right-to-work states, were included to proxy for collective bargaining status on the predisplacement job. The occupation unemployment rate, state or SMSA unemployment rate, and the average industry employment growth rate were added to account for differences in economic conditions across localities and employment sectors. Variables measuring the predisplacement wage residual and predicted probability of receiving unemployment insurance benefits (conditional on experiencing some unemployment) were also included.[1] A complete description of the variables used in this analysis, along with the data sources for the merged variables, are provided in Figure 2.1.

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[1] Some previous researchers (e.g. Addison & Portugal 1987a) have controlled for the actual receipt of unemployment insurance rather than the conditional probability. The receipt of UI is endogenous, however, since workers avoiding nonemployment are generally ineligible for benefits. Workers receiving benefits will therefore almost certainly have longer average joblessness, even in the absence of a true UI effect.

The 1988 DWS is clearly the best data source currently available for the study of displaced workers; nonetheless, it does contain a number shortcomings which should be noted. First, although fairly detailed information is available on worker characteristics, very little firm level data is included. For example, we do not know the size of the company, collective bargaining status of the employee, or anything about the financial status of the employer. As mentioned above, industry, occupation, and state level variables have been obtained from other sources and merged with the DWS data to proxy for these variables. This minimizes but by no means eliminates the problem.

Second, the DWS contains retrospective information on predisplacement wages and joblessness over as much as a five year period. Retrospective data is subject to well known "recall biases" which increase with the amount of time since the event in question has occurred.[2] A similar problem is that, because the DWS does not obtain retrospective information for nondisplaced workers, it is generally not possible to construct a control group of job stayers.[3] The analysis of displaced workers would ideally be undertaken using panel data. As explained in some detail by Ruhm (1989a), this is particularly important for investigating the time profile of displacement induced changes. With a single wave of the DWS, there is perfect collinearity between the amount of time since displacement and the year of job loss. Thus, timing effects can not be separated from the impact of economic conditions at the date of termination. In principle, this difficulty could be overcome by merging data from several waves of the DWS. Unfortunately inconsistencies in the type of data collected across surveys, particularly regarding joblessness, make this problematic.

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[2] Horvath (1982) and Akerlof & Yellen (1985) provide careful analyses of the importance of recall bias in unemployment data. Seitchik (1989) suggests that these biases are also significant for the earlier versions of the DWS.

[3] This problem is discussed extensively in Ruhm (1989b). The best effort to assemble an appropriate comparison group using DWS data is by Madden (1988).

Third, the precise information obtained by the 1988 DWS is deficient in several areas. Respondents are asked how many weeks they were out-of-work in their initial spell of joblessness but no differentiation is made between unemployment and periods of labor force nonparticipation. Although some researchers (i.e. Clark & Summers, 1982) have suggested that there is often no clear distinction between the two labor market states, it would still be preferable to have them separated.

A more serious concern is the continued inadequacy of the advance notice data. Although the 1988 DWS improves upon previous surveys, by adding questions on written advance notice, there is still no way to determine whether respondents expecting their jobs to terminate in the absence of written advance notice did so because they obtained specific *verbal* announcements or if they did so without any form of early warning. As shown below, distinguishing between these two possibilities is important. The data also provides no indication to what extent formally notified workers first received informal information.

Finally, the 1988 DWS, like its predecessors, identifies geographic location at the survey date rather than the time of displacement. Since approximately 19 percent of the sample changes location between the latter and former periods, this could lead to biased estimates of local labor market effects. To the extent that respondents are more likely to move out of depressed areas and into locations with low unemployment, the impact of regional conditions is likely to be understated. Ehrenberg & Jakubson (1988) and Howland & Peterson (1988) compare estimates with and without movers (for the 1984 DWS) and conclude that these biases are quite small. Nonetheless, the analysis of local economic effects should be interpreted with caution.

2.2 Frequency of Advance Notice

Tables 2.1 and 2.2 indicate the frequency with which displaced workers in the United States received various kinds of prenotification in the 1983 through 1987 time period, prior to passage of mandatory advance notice legislation. The first table shows proportions weighted by the inverse probability of selection into the sample and thus presents nationally representative statistics. The second table shows raw (unweighted) proportions. The weighted and unweighted percentages are virtually identical in all cases.

A slight majority (52.3 percent) of displaced workers anticipated or received prior notice of their job loss (Table 2.1, column 1). This does not imply that formal advance notice was common. Only 15.1 percent of dislocated individuals obtained written notice of any type and just 5.1 percent were provided with formal announcements at least 2 months before the termination (columns 5 through 7). Substantial early notice was more common for workers displaced by plant closings than for those involved in partial layoffs — 12.4 percent of the former group received at at least 1 month of written notice versus 5.7 percent of the latter.

The percentage of respondents in the 1988 DWS who expected their layoffs is similar to the 55 and 56 percent, respectively, noted by Ehrenberg & Jakubson (1988) for the 1984 and 1986 DWS. The proportion with written advance notice is also close to that found in a General Accounting Office survey of representative establishments. Analysis of the GAO data revealed that 81 percent of workers received less than 1 month's warning and only 5 percent obtained over 90 days notice in 1983 and 1984 (U.S. General Accounting Office, 1987).[4] The GAO defines notice to include specific information (whether verbal or written) concerning the date of displacement and workers affected. The similarity of the GAO numbers to those for written notification in the DWS therefore suggests that precise verbal information is rarely provided.[5] This implies that most workers who "expected" their jobs to terminate, in the absence of formal notice, probably also lacked specific unwritten information on when and whether the layoff would occur.

Tables 2.3 and 2.4 indicate the weighted and unweighted variable means for subsamples receiving various types of advance notice. A comparison of columns 2 and 3 of Table 2.3 shows that notified workers typically had considerably greater job seniority than non-notified individuals (5.8 versus 4.5 years), were much more likely to be covered by group health insurance plans (69.6 versus 58.8 percent), and more often were involved in plant closings (59.2 versus 45.2 percent). They were also somewhat older, more often married or nonwhite, relatively frequently worked in slow growing industries or occupations, and lost positions industries, occupations, and states with relatively high unionization rates. There was no difference in family sizes, predisplacement wages, or local unemployment rates between notified and non-notified individuals.

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[4] See Brown (1987) for further discussion of the GAO study. A recent Conference Board Survey (Berenbeim 1986) suggests that advance notice is provided more frequently. These findings are questionable, however, because of the non-representativeness of the sample and frequency of nonresponse.

[5] If specific unwritten notice were provided with any frequency, the GAO percentages would be much higher than those for written notice in the DWS.

The distinction between formal and informal notice is sometimes important. For instance, nonwhites are less likely than whites to expect displacements but receive written notification more frequently. The formal announcements received by minorities are typically of short duration, however, and so are not likely to be very helpful in easing adjustment problems. Probabilities of obtaining written notice more than 2 months before displacement are relatively high for married workers, females, full-time employees, respondents with long job seniority or in multiple earner households, those covered by group health insurance plans, and persons affected by plant closings. Thus, 89.9 percent of displaced persons receiving more than 2 months written notice left positions providing group health insurance, 68.8 percent were married, 72.2 involved in plant closings, and their average job tenure was 8.9 years. Corresponding percentages for non-notified respondents were 58.8, 62.2, and 45.2 percent, respectively, with mean seniority of just 4.5 years (see Table 2.3, columns 2 and 7).

Prenotified workers relatively frequently lost jobs in highly unionized industries, occupation, and states. The average worker receiving formal or informal notice left a job with industry and occupation unionization rates of 25.6 and 25.3 percent respectively; the corresponding probabilities for non-notified respondents were 23.7 and 23.1 percent (see rows 17 and 18).

Similarly, notified individuals work in states with relatively high unionization rates and comparatively rarely in states with right-to-work laws (19 and 20). These findings accord with the common belief that unionized workers receive more extensive information on impending job displacements than do nonunionized individuals. Interestingly, the state and industry unionization percentages are highly related to the probability of receiving lengthy written advance notice (>2 months) but the occupation rate is less strongly correlated.

Local and sectoral economic conditions have a mixed impact on prenotification probabilities. Individuals displaced from occupations or residing in local labor markets with low unemployment rates receive unwritten advance notice fairly infrequently but obtain long durations of written notice relatively often (rows 16 and 21). Conversely, job loss out of a slow growing industry is associated with higher probabilities of obtaining all types of notice (row 15).

2.3 Consequences of Advance Notice

Most workers experience significant joblessness following permanent job loss. Ruhm (1989a) calculates that the average displacement leads to a more than 13 week increase in nonemployment over a 2 year period. Adding in the extra time out-of-work during periods of labor force nonparticipation and that which would have occurred in the absence of the termination (e.g. during temporary layoffs), total postdisplacement joblessness is significantly greater.

Media coverage of displacement induced wage changes typically emphasizes the large earnings losses suffered by dislocated individuals. Unfortunately, some economists (e.g. Bluestone & Harrison 1982) have promoted relatively simplistic stories where displacement implies uniformly large income losses. In reality, however, the interaction between permanent layoffs and subsequent earnings is much more complicated. Between one third and half of involuntary job changers earn higher wages in their new jobs than their old, while a quarter to a third suffer 25 percent or larger earnings reductions (Podgursky and Swaim 1987b, Ruhm 1987a). Thus, at least as many individuals benefit, in monetary terms, as experience large losses. Given these disparate experiences, it is unfortunate that the impact of advance notice for postlayoff earnings has received scant attention in previous research.

Descriptive information on postdisplacement joblessness, wage changes, and reemployment probabilities is provided below. Subsequent chapters employ regression analysis which includes controls for a wide variety of covariates. Tables 2.5 and 2.6 present weighted and unweighted statistics on average joblessness and wage changes for subsamples stratified by prenotification status. The weighted and unweighted percentages are again similar. Throughout the remainder of this report, we focus on the raw sample statistics rather than using weighted data.

The first panel of Table 2.6 presents information on expected postdisplacement joblessness. Respondents average 27.4 weeks out-of-work in their initial spell of nonemployment (Table 2.6, row 1).[6] This slightly understates the true extent of joblessness since durations are top-coded at 99 weeks and are right-censored for the 19.3 percent of the sample continuously out-of-work between the date of the employment termination and January 1988. Females, nonwhites, and white collar workers are out-of-work for longer than their counterparts. Surprisingly, joblessness is also slightly more extended for persons displaced as the result of plant closings than for those involved in partial layoffs. Workers expecting job terminations have slightly shorter nonemployment than those who do not — 26.7 versus 28.2 weeks — with the largest reductions for males, blue collar workers, and those displaced by plant closings (see columns 2 and 3).

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[6] Similar findings for the 1984 and 1986 DWS are presented in Flaim & Sehgal (1985); Horvath (1987); and Podgursky & Swaim (1987a).

The second panel of Table 2.6 shows average changes in real weekly wages between the time of displacement and the survey date for workers who are reemployed in January 1988. The typical displaced worker earns 8.53 percent more at the survey date than at the conclusion of the predisplacement job, with especially rapid wage growth experienced by females, nonwhites, white collar employees, and those losing jobs because of plant closings (see column 1). Wage outcomes are more favorable for prenotified job losers. These individuals are paid 12.32 percent more preseparation wage levels in January 1988, while the increase is only a third as large (4.12 percent) for those lacking advance warning (row 10). This wage advantage for notified individuals is important for all population subgroups.

A number of previous researchers (e.g. Swaim & Podgursky, 1989) have hypothesized that advance notice should yield the greatest benefits when it is received in writing and well before the job termination. Written notice is thought to be more efficacious than verbal warnings because it provides more precise information on the timing and nature of impending terminations. Lengthy notice is believed to allow considerable predisplacement job search and possibly to permit workers to begin training for new positions prior to leaving their old jobs.[7] Columns 4 through 7 provide the first indication that these simple stories may be inadequate.

Comparing column 4 with columns 5 through 7 reveals that the shortest average durations of joblessness and the greatest mean wage gains were experienced by workers anticipating displacements in the absence of written notice. The average initial nonemployment spell lasted 25.85 weeks for this group but was 2.9 to 3.9 weeks longer for formally notified respondents. Workers receiving written warnings actually experienced greater mean joblessness than those surprised by the terminations. Similarly, respondents receiving unwritten warning of permanent layoffs have substantially larger average wage gains than either the group lacking notice (4.12%) or those with any category of written notification (-5.55 to 5.32 percent). A similar pattern is observed when the sample is stratified by sex, race, occupation, or reason for displacement.

Just as there is no evidence that formal notification is more beneficial than unwritten notice, the sample means fail to indicate improved outcomes for workers with longer durations of formal notice. Workers receiving written announcements more than 2 months before the layoff date are jobless longer than any other group, while the wages of individuals with less than 1 month of notice grow faster than for those with more extensive written warning (see Table 2.6 rows 1 and 10).

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[7] Hamermesh (1987) has also suggested that this may allow workers to switch from firm-specific to general skill training.

Advance notice may effect the distribution as well as the mean of joblessness and wage growth. For example, previous research (see Addison & Portugal, 1987; Ehrenberg & Jakubson, 1988) suggests that prenotification increases the probability that workers avoid joblessness altogether, while having little impact on the nonemployment durations of workers who fail to do so. With this in mind, Tables 2.7 through 2.15 show the relationship between advance notice and the distribution of both joblessness and wage changes for subsamples stratified by race, sex, occupation, and the reason for displacement. Also included in the tables is information on the labor force status of displaced workers in January 1988. This may be important if, independent of its effect on the mean or variance of the initial spell of nonemployment, prior notice impacts upon future reemployment or labor force participation probabilities.

Table 2.7 shows results for the full sample. There is again strong evidence that early warning of displacement increases probabilities of avoiding joblessness. Only 9.6 percent of non-notified workers move directly into new positions (within 1 week of leaving the old ones) versus 13.9 percent of the prenotified individuals (row 5). The increase is even larger for individuals formally notified more than a month before displacement — 16.1 percent of workers with 1 to 2 months written notice are out-of-work less than a week and 20.4 percent of those notified more than 2 months ahead of time.

The lengthy average joblessness of formally notified workers is the result of two factors. First, the group receiving less than 1 month of written notice has uniformly high levels of unemployment. Only 8.9 percent of these individuals are jobless less than 1 week and 36.9 percent are out-of-work 6 months or more (column 5). Second, the superior ability of workers with longer written notice to avoid time out-of-work altogether is more than offset by their relatively high probabilities of experiencing extended nonemployment. Thus, where the probabilities of being jobless at least 6 months are 33.5 and 32.3 percent for the non-notified and informally warned groups, 37.8 percent of persons with more than 2 months written warning experience similarly lengthy nonemployment (rows 8 and 9).

The wage distributions (rows 12 through 16) indicate that that advance notice has a more uniform impact on survey date earnings than on postdisplacement joblessness. The wages of informally notified respondents grow faster than for any other category because they are least likely to experience 10 percent or greater wage cuts and, with the exception of the group formally notified more than 2 months in advance of the termination, are most likely to obtain pay raises exceeding 10 percent. Conversely, the poor performance of workers with 1 to 2 months advance notice occurs because they both have the highest probability of wage cuts exceeding 25 percent and least often receive equivalent size pay increases. On the other hand, workers with more than 2 months written notice have the highest probabilities of earning more than 75 and 110 percent of their predisplacement wages, the lowest of taking 25 percent or larger pay reduction but have fairly small probabilities of receiving raises exceeding 25 percent.

For the most part, these patterns hold in the stratified subsamples (see tables 2.8 through 2.15). There is some evidence, however, that lengthy advance notice is associated with relatively high probabilities of substantial wage increases for females, nonwhites, blue collar workers, and those involved in plant closings. Minority workers also accrue substantial benefits from lengthy written notice, in the form of reduced joblessness, particularly due to a substantial increase in the likelihood of avoiding all nonemployment and spells exceeding 6 months.

Notified workers are somewhat more likely to be employed and to participate in the labor force in January 1988 than their counterparts (Table 2.7, rows 1 through 3). The increase is larger for women than men and the employment effect is especially dramatic for nonwhites (see Tables 2.8, 2.9, and 2.13).

2.4 Summary

This chapter presents descriptive information on the data set used in the analysis, the frequency of advance notice, and on the relationship between notification status and postdisplacement joblessness, wage changes, and survey date labor force status. Although a slight majority of respondents anticipated job losses, the vast majority did so in the absence of written notice. Informal notice is associated with relatively brief initial spells of nonemployment. Written notification, although it increases the likelihood that individuals move into new positions without intervening joblessness, does not imply speedier average reemployment. Similarly, the average wage gains experienced by these individuals are modest or nonexistent.

The most striking finding in the chapter is the superior performance of informally notified workers compared to either their non-notified counterparts or to those receiving written notice. Although these results are obviously preliminary, pending the more detailed regression analysis of future chapters, they suggest important problems with previous research examining the effects of advance notice.

Absent any theoretical reason why formal notice should *extend* joblessness and *retard* wage growth, we anticipate that persons notified in writing will do at least as well as those receiving unwritten notice. One reason this does not occur may be the extremely vague definition of informal notification in the *DWS*, which includes "expectations" of job terminations in the absence of any notice, as well as verbal warnings. This could lead to biases if, for example, better informed workers are more likely to both anticipate job loss and to quickly find new high paying jobs once it occurs. Alternatively, workers may be more likely to receive written advance notice in situations in situations where equivalent new jobs are hard to come by.

Figure 2.1: Description of Variables Used in Analysis

Variables Obtained from CPS-DWS

Continuous Variables

EXP = Years of Potential Labor Market Experience (Age - Education - 6)

EXPSQ = EXP * EXP

EDUC = Years of Schooling (censored at 17 years)

EDUCSQ = EDUC * EDUC

TENURE = Number of years working for the predisplacement employer

TENSQ = TENURE * TENURE

NUMCHILD = Number of children under the age of eighteen

NUMEARN = Number of earners (16+) in family

Dummy Variables (equal to one if:)

MARRIED = Married with spouse living in household

HEAD = Head of household

FEMALE = Female

BLACK = Black

OTHRACE = Hispanic, Asian, or other (nonblack) nonwhite

PRTPREV = Part-time work (self-defined) in predisplacement job

HINS = Included in group health insurance plan in pre-displacement job

CLOSING = Job terminated due to plant closure or relocation

MANDSTE = Resides in state with mandatory advance notice legislation (Maine, Wisconsin, or Hawaii)

VOLSTE = Resides in state with program to encourage voluntary advance notice (Massachusetts, Michigan, or Maryland).

EXPECT = Displacement "expected" or advance notice received

WRITTEN = Received written advance notice of displacement

WRITGT1 = Received > 1 month written advance notice of displacement

WRITGT2 = Received > 2 months written advance notice of displacement

Variables Obtained From Other Sources or Constructed

STEUNION = State Unionization Rate in 1982 (source: Statistical Abstract, 1988)

RTWORK = dummy variable indicating residence in "right-to-work" state (source: Statistical Abstract, 1988)

URATE = national unemployment rate for the year of displacement

AREARATE = unemployment rate in year of displacement for SMSA of residence (if resides in one of the 50 largest) or state unemployment rate otherwise (source: Employment and Earnings, May 1982-8)

INDCHG = Ave. annual growth rate of industry employment (19 industry categories), 1980-6 (source: Statistical Abstract, 1988)

OCCCRATE = Unemployment Rate by occupation (12 categories) for 1986 (source: Statistical Abstract, 1988)

INDUNION = Ave. Unionization rate by industry (19 categories) for 1978-80 (source: Kokkenlenberg and Sockell, 1985)

OCCUNION = Ave. Unionization rate by occupation (12 categories) for 1978-80 (source: Kokkenlenberg and Sockell, 1985)

RESID = Wage residual: obtained from subtracting actual log real wage from predicted value obtained from an earnings regression with controls for individual characteristics and state fixed effects (but not for industry or occupation characteristics)

UIPROB = Probability of being eligible for unemployment benefits: obtained from probit regression estimated for persons with >1 week of postdisplacement unemployment; regressors include individual characteristics, previous wages, and fixed effects for the year of displacement (4 variables), 48 industries, 44 occupations, and 50 states

Table 2.1: Probability of Receiving Advance Notice
(weighted)

	Sample Size (1000's)	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
All Workers	6898	47.7%	52.3%	37.2%	5.8%	4.2%	5.1%
Males	4014	48.3	51.6	36.9	5.8	4.0	4.8
Females	2884	46.8	53.2	37.6	5.8	4.4	5.5
Plant Closings	3625	41.0	58.9	41.8	4.6	5.4	7.0
Layoffs	3273	55.1	44.9	32.0	7.1	2.7	3.0
Whites	5869	47.0	53.0	38.0	5.6	4.1	5.3
Blacks	828	51.6	48.3	34.1	6.6	4.2	3.5
Other Nonwhites	201	52.7	47.3	25.4	8.9	5.6	7.3
White Collar	4321	49.4	50.5	35.9	5.0	4.2	5.4
Blue Collar	2577	44.8	55.2	39.3	7.1	4.1	4.7
Professional	1217	49.1	50.9	34.1	4.0	4.8	8.1

Table 2.2:
Probability of Receiving Advance Notice
(unweighted)

	Sample Size	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					<1	1-2	>2
All Workers	4058	47.0%	53.0%	37.9%	6.1%	4.0%	5.0%
Males	2322	47.6	52.4	37.5	6.2	4.0	4.8
Females	1736	46.2	53.7	38.4	6.1	4.1	5.1
Plant Closings	2115	40.4	59.6	42.6	5.1	5.1	6.9
Layoffs	1943	54.2	45.8	32.8	7.3	2.9	2.8
Whites	3529	46.6	53.4	38.6	5.9	3.9	5.0
Blacks	402	49.8	50.2	34.3	7.2	4.5	4.2
Other Nonwhites	127	51.2	48.9	28.3	8.7	4.7	7.1
White Collar	2545	48.4	51.6	36.9	5.4	4.2	5.1
Blue Collar	1513	44.7	55.3	39.5	7.3	3.8	4.6
Professional	705	48.1	51.9	35.9	4.3	4.4	7.4

Table 2.3:
Mean Characteristics by Type of Advance Notice:
(weighted)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Personal Characteristics							
Age (years)	38.0	37.7	38.3	38.2	37.3	38.5	39.9
Education (years)	12.9	13.0	12.8	12.7	12.8	13.1	13.3
Married (%)	64.4	62.2	66.4	66.8	65.7	61.0	68.8
Household Head (%)	64.6	65.6	63.7	62.3	68.0	70.8	63.0
Nonwhite (%)	14.9	16.2	13.7	13.0	18.1	16.1	12.3
Female (%)	41.8	41.0	42.6	42.3	41.7	43.6	45.0
# Children	1.73	1.73	1.73	1.72	1.77	1.76	1.74
# Earners	1.19	1.15	1.24	1.22	1.21	1.23	1.44
Pre-Displacement Job Characteristics							
Seniority (years)	5.2	4.5	5.8	5.3	5.4	7.1	8.9
Health Insurance (%)	64.5	58.8	69.6	64.6	77.0	79.5	89.9
Log Weekly Wage	5.67	5.66	5.67	5.60	5.74	5.83	5.92
Plant Closing (%)	52.6	45.2	59.2	59.1	41.9	68.5	72.2
Blue Collar (%)	37.4	35.1	39.4	39.6	45.8	36.4	33.8
Part-time (%)	9.17	8.71	9.58	11.1	6.44	7.73	4.07
Industry Employment Growth (%)	1.14	1.32	0.98	1.03	0.81	1.14	0.70
Occupation Unemployment Rate (%)	6.63	6.48	6.76	6.79	7.45	6.61	5.91
Industry Unionization Rate (%)	24.7	23.7	25.6	25.2	28.5	24.5	26.4
Occupation Unionization Rate (%)	24.3	23.1	25.4	25.1	28.3	24.3	24.6
Geographic Characteristics							
State Unionization Rate (%)	20.8	20.5	21.0	20.6	21.2	21.5	23.1
Right-to-Work State (%)	33.3	34.2	32.5	34.8	32.1	27.7	20.2
Area Unemployment Rate (%)	7.42	7.42	7.42	7.44	7.44	7.51	7.18

Table 2.4:
Mean Characteristics by Type of Advance Notice:
(unweighted)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	>2
Personal Characteristics							
Age (years)	38.2	37.8	38.5	38.4	37.6	38.7	40.1
Education (years)	12.9	13.0	12.9	12.8	12.9	13.1	13.2
Married (%)	65.4	63.3	67.3	67.7	66.7	61.4	69.7
Household Head (%)	64.6	65.8	63.6	61.9	68.3	73.0	63.2
Nonwhite (%)	13.4	13.9	12.2	11.3	16.1	14.7	12.9
Female (%)	42.8	42.1	43.4	43.4	42.6	43.6	44.3
# Children	1.74	1.73	1.74	1.72	1.84	1.83	1.72
# Earners	1.19	1.14	1.23	1.22	1.21	1.16	1.42
Pre-Displacement Job Characteristics							
Seniority (years)	5.2	4.5	5.9	5.4	5.4	6.7	9.3
Health Insurance (%)	65.0	59.0	70.3	65.8	77.5	77.3	89.6
Log Weekly Wage	5.67	5.66	5.67	5.62	5.74	5.82	5.90
Plant Closing (%)	52.1	44.8	58.6	58.6	43.0	65.6	72.6
Blue Collar (%)	37.3	35.5	38.9	38.9	44.6	35.0	34.8
Part-time (%)	9.55	9.35	9.73	11.2	6.58	8.28	3.76
Industry Employment Growth (%)	1.08	1.24	0.94	1.01	0.60	1.21	0.64
Occupation Unemployment Rate (%)	6.58	6.26	6.69	6.69	7.46	6.33	6.03
Industry Unionization Rate (%)	24.9	23.8	25.9	25.4	29.3	24.5	27.1
Occupation Unionization Rate (%)	24.3	23.4	25.2	24.8	28.4	24.0	25.0
Geographic Characteristics							
State Unionization Rate (%)	20.5	20.3	20.7	20.3	21.0	21.5	23.1
Right-to-Work State (%)	33.8	34.6	33.0	35.5	33.3	25.1	20.4
Area Unemployment Rate (%)	7.39	7.42	7.36	7.39	7.28	7.61	7.09

Table 2.5:
Average Post-Displacement Joblessness and Wage Changes by Type of Advance Notice
(weighted)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Weeks of Joblessness [a]							
All Workers	27.37	28.37	26.46	25.94	28.49	27.75	26.89
Males	23.28	24.08	22.54	21.40	26.12	26.52	23.46
Females	33.02	34.52	31.71	32.06	31.81	29.32	31.09
Plant Closings	27.35	28.81	26.34	26.76	25.18	27.24	23.92
Layoffs	27.40	28.02	26.65	24.76	30.87	28.89	34.61
Whites	25.82	26.90	24.87	24.51	24.45	26.76	26.33
Nonwhites	36.18	35.93	36.45	35.39	47.11	32.84	30.92
White Collar	29.15	29.33	28.97	29.23	33.08	22.54	28.47
Blue Collar	24.39	26.59	22.62	20.90	23.05	36.74	23.79
Percent Wage Changes [b]							
All Workers	9.13%	4.53%	13.18%	19.11%	5.73%	-9.26%	1.71%
Males	6.71	3.56	9.66	14.41	9.12	-11.68	-5.06
Females	12.85	6.15	18.26	26.09	0.15	-6.04	9.18
Plant Closings	14.49	5.89	20.75	28.58	15.69	-7.30	5.27
Layoffs	2.80	5.79	1.71	4.69	-0.96	-13.13	-2.71
Whites	9.54	5.89	13.04	19.66	0.93	-10.28	0.92
Nonwhites	6.57	2.93	14.15	15.07	29.69	-3.39	6.12
White Collar	12.52	4.69	19.31	27.34	12.69	-7.67	0.84
Blue Collar	4.00	5.89	4.74	8.07	-1.57	-11.79	3.45

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.6:
Average Post-Displacement Joblessness and Wage Changes by Type of Advance Notice (unweighted)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Weeks of Joblessness [a]							
All Workers	27.40	28.16	26.73	25.85	28.51	28.46	29.75
Males	23.25	24.26	22.33	20.88	26.33	27.75	23.92
Females	32.91	33.48	32.43	32.28	31.46	29.37	37.09
Plant Closings	27.83	29.13	26.95	26.25	28.50	29.33	28.30
Layoffs	26.94	27.38	26.42	25.29	28.52	26.75	33.60
Whites	25.87	26.66	25.18	24.25	26.11	26.74	29.72
Nonwhites	37.57	37.38	37.75	38.31	41.31	36.42	29.96
White Collar	29.23	29.25	29.22	28.62	33.47	24.36	32.80
Blue Collar	24.32	26.18	22.83	21.49	22.33	36.00	24.04
Percent Wage Changes [b]							
All Workers	8.53%	4.12%	12.32%	16.93%	5.32%	-5.55%	2.42%
Males	6.16	2.31	9.61	13.35	8.42	-5.46	-4.06
Females	11.98	6.91	16.05	21.90	0.50	-5.66	10.59
Plant Closings	12.33	4.80	17.37	22.89	11.46	-2.62	4.15
Layoffs	4.11	3.51	4.78	7.94	1.05	-11.33	-2.71
Whites	8.47	4.63	11.73	16.73	2.29	-6.40	0.45
Nonwhites	8.94	0.59	16.80	18.71	22.86	-0.17	13.38
White Collar	11.88	6.24	17.07	23.29	8.81	-5.39	0.71
Blue Collar	3.48	0.58	5.71	8.07	1.75	-8.60	5.48

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.7:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: All Displaced Workers
(n = 4058)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	47.0%	53.0%	37.9%	6.1%	4.0%	5.0%
Employment Status							
Employed	73.2	71.5	74.7	74.8	72.7	79.1	73.1
Unemployed	15.4	16.6	14.3	14.1	15.7	9.8	17.4
Out of Labor Force	11.5	12.0	11.0	11.1	11.7	11.0	9.5
Weeks of Joblessness[a]							
< 1	11.9	9.6	13.9	13.7	8.9	16.1	20.4
1 - 4	22.0	23.0	21.0	21.8	21.9	17.9	16.9
5 - 12	18.2	20.0	16.5	16.9	18.6	15.3	11.9
13 - 26	14.5	13.9	15.1	15.4	13.8	16.7	12.9
27 - 52	20.6	20.1	21.1	20.4	23.1	22.2	22.9
> 52	12.8	13.4	12.4	11.9	13.8	11.7	14.9
Censored	19.3	19.8	18.8	18.6	20.2	16.7	20.9
Wage Ratio[b]							
< 0.75	29.5	30.5	28.5	28.7	27.6	31.5	26.2
0.75 - 0.9	12.5	12.0	13.0	11.7	20.5	13.0	13.9
0.9 - 1.1	23.4	23.5	23.4	23.5	18.6	30.6	23.1
1.1 - 1.25	11.1	9.8	12.2	12.6	7.1	10.2	16.9
> 1.25	23.5	24.3	22.8	23.6	26.3	14.8	20.0

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.8:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Males
(n = 2322)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	47.6%	52.4%	37.5%	6.2%	4.0%	4.8%
Employment Status							
Employed	77.8	76.4	79.1	76.4	76.9	83.7	73.2
Unemployed	16.1	17.5	14.8	17.5	14.7	8.7	23.2
Out of Labor Force	6.2	6.2	6.2	6.2	8.4	7.6	3.6
Weeks of Joblessness[a]							
< 1	13.5	10.7	16.0	16.5	11.3	15.4	18.8
1 - 4	24.1	25.2	23.2	24.5	22.5	16.5	19.6
5 - 12	18.0	19.7	16.5	16.5	21.8	15.4	10.7
13 - 26	14.8	14.5	15.1	14.7	14.8	19.8	14.3
27 - 52	19.6	19.4	19.8	19.3	16.9	22.0	25.0
> 52	10.0	10.6	9.5	8.6	12.7	11.0	11.6
Censored	15.0	15.4	14.7	14.2	14.8	12.1	20.5
Wage Ratio[b]							
< 0.75	30.3	31.6	29.0	29.0	25.3	34.4	29.2
0.75 - 0.9	12.6	13.1	12.2	11.2	16.8	9.8	15.3
0.9 - 1.1	23.7	23.0	24.4	24.5	22.1	29.5	22.2
1.1 - 1.25	11.1	9.9	12.2	12.0	8.4	13.1	18.1
> 1.25	22.3	22.4	22.3	23.4	27.4	13.1	15.3

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.9:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Females
(n = 1736)

	All Workers	No Notice	Received Notice	Type of Notice: Unwritten	Written (# months) ≤1	1-2	≥2
Total	100.0%	46.2%	53.7%	38.4%	6.1%	4.1%	5.1%
Employment Status							
Employed	67.1	64.8	69.0	68.4	67.0	73.2	73.0
Unemployed	14.4	15.3	13.6	13.8	17.0	11.3	10.1
Out of Labor Force	18.6	19.9	17.4	17.8	16.0	15.5	16.9
Weeks of Joblessness[a]							
< 1	9.8	8.1	11.3	10.0	5.7	16.9	22.5
1 - 4	19.1	20.1	18.2	18.2	21.0	19.7	13.5
5 - 12	18.4	20.5	16.6	17.5	14.3	15.5	13.5
13 - 26	14.1	13.0	15.1	16.3	12.4	12.7	11.2
27 - 52	22.1	21.1	22.9	21.9	31.4	22.5	20.2
> 52	16.6	17.2	16.0	16.1	15.2	12.7	19.1
Censored	25.0	25.9	24.2	24.2	27.6	22.5	21.4
Wage Ratio[b]							
< 0.75	28.3	28.9	27.9	28.2	31.2	27.7	22.4
0.75 - 0.9	12.4	10.2	14.2	12.3	26.2	17.0	12.1
0.9 - 1.1	23.1	24.2	22.1	22.1	13.1	31.9	24.1
1.1 - 1.25	11.0	9.6	12.2	13.6	4.9	6.4	15.5
> 1.25	25.2	27.1	23.4	23.9	24.6	17.0	25.9

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.10:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Plant Closings
(n = 2115)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	40.4%	59.6%	42.6%	5.1%	5.1%	6.9%
Employment Status							
Employed	75.3	73.8	76.4	77.3	70.1	78.5	73.3
Unemployed	12.7	13.3	12.3	11.6	15.0	9.4	17.1
Out of Labor Force	12.0	12.9	11.4	11.1	15.0	12.2	9.6
Weeks of Joblessness[a]							
< 1	14.2	11.0	16.3	16.6	10.4	15.0	19.9
1 - 4	23.0	26.3	20.7	21.7	17.9	15.9	20.6
5 - 12	17.4	18.7	16.5	16.5	20.8	18.7	11.6
13 - 26	14.7	13.5	15.5	15.2	17.9	19.6	12.3
27 - 52	17.9	16.3	18.9	17.7	21.7	19.6	23.3
> 52	12.9	14.2	12.1	12.3	11.3	11.2	12.3
Censored	17.2	17.4	17.0	16.4	17.9	17.8	19.9
Wage Ratio[b]							
< 0.75	28.9	30.5	27.9	28.6	26.6	29.6	23.7
0.75 - 0.9	12.8	12.5	13.0	12.3	20.3	9.9	14.4
0.9 - 1.1	22.9	23.5	22.9	22.3	18.8	33.8	20.6
1.1 - 1.25	12.0	9.2	13.9	13.9	6.3	12.7	19.7
> 1.25	23.4	24.9	22.4	22.9	28.1	14.1	21.7

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.11:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Layoffs
(n = 1943)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	54.2%	45.8%	32.8%	7.3%	2.9%	2.8%
Employment Status							
Employed	70.9	69.6	72.4	71.1	74.7	80.4	72.7
Unemployed	18.2	19.2	17.1	17.7	16.2	10.7	18.2
Out of Labor Force	10.9	11.2	10.6	11.2	9.2	8.9	9.1
Weeks of Joblessness[a]							
< 1	9.4	8.4	10.6	9.6	7.8	18.2	21.8
1 - 4	20.9	20.4	21.4	21.8	24.8	21.8	7.3
5 - 12	19.0	21.1	16.6	17.5	17.0	9.1	12.7
13 - 26	14.4	14.3	14.5	15.6	10.6	10.9	14.6
27 - 52	23.6	23.1	24.2	24.2	24.1	27.3	21.8
> 52	12.7	12.7	12.7	11.3	15.6	12.7	21.8
Censored	21.6	21.8	21.4	21.7	22.0	14.6	23.6
Wage Ratio[b]							
< 0.75	30.1	30.6	29.5	28.9	28.3	35.1	33.3
0.75 - 0.9	12.2	11.5	13.1	10.7	20.7	18.9	12.1
0.9 - 1.1	24.1	23.9	24.3	25.1	18.5	24.3	30.3
1.1 - 1.25	10.0	10.3	9.7	10.7	7.6	5.4	9.1
> 1.25	23.6	23.7	23.5	24.6	25.0	16.2	15.2

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.12:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Whites
(n = 3529)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	46.6%	53.4%	38.6%	5.9%	3.9%	5.0%
Employment Status							
Employed	74.6	73.2	75.7	75.9	74.2	81.3	72.0
Unemployed	14.1	15.0	13.3	13.1	13.4	7.9	18.9
Out of Labor Force	11.4	11.8	11.0	11.0	12.4	10.8	9.1
Weeks of Joblessness[a]							
< 1	12.7	10.4	14.7	14.4	9.6	18.1	20.0
1 - 4	22.2	23.4	21.1	22.0	21.2	17.4	17.1
5 - 12	18.7	20.8	17.0	17.6	18.8	16.0	10.9
13 - 26	15.0	14.5	15.5	15.6	15.4	16.7	13.1
27 - 52	19.7	18.8	20.4	19.7	22.1	20.3	23.4
> 52	11.8	12.2	11.5	10.7	13.0	11.6	15.4
Censored	17.4	17.5	17.3	16.0	17.3	15.2	21.1
Wage Ratio[b]							
< 0.75	29.7	30.3	29.1	28.7	27.8	33.3	30.0
0.75 - 0.9	12.7	12.4	12.9	11.7	21.1	11.8	13.6
0.9 - 1.1	23.3	23.6	23.1	23.1	19.6	31.2	20.0
1.1 - 1.25	10.7	9.5	11.7	12.5	6.8	8.6	14.6
> 1.25	23.7	24.3	23.2	24.0	24.8	15.1	21.8

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.13:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Nonwhites
(n = 529)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	50.1%	49.9%	32.9%	7.6%	4.5%	4.9%
Employment Status							
Employed	64.1	60.8	67.4	66.1	65.0	66.7	80.8
Unemployed	23.8	26.4	21.2	21.8	27.5	20.8	7.7
Out of Labor Force	12.1	12.8	11.4	12.1	7.5	12.5	11.5
Weeks of Joblessness[a]							
< 1	6.7	4.6	8.8	8.1	5.1	4.2	23.1
1 - 4	20.7	21.0	20.3	19.8	25.6	20.8	15.4
5 - 12	14.5	15.7	13.4	11.6	18.0	12.5	19.2
13 - 26	11.3	10.3	12.3	13.4	5.1	16.7	11.5
27 - 52	27.2	27.9	26.4	26.2	28.2	33.3	19.2
> 52	19.7	20.6	18.8	20.9	18.0	12.5	11.5
Censored	31.9	34.0	29.9	30.8	35.9	25.0	19.2
Wage Ratio[b]							
< 0.75	28.0	31.9	24.2	28.4	26.1	20.0	5.0
0.75 - 0.9	11.5	9.0	13.7	11.6	17.4	20.0	15.0
0.9 - 1.1	24.6	22.9	26.1	26.3	13.0	26.7	40.0
1.1 - 1.25	13.8	11.8	15.7	13.7	8.7	20.0	30.0
> 1.25	22.2	24.3	20.3	20.0	34.8	13.3	10.0

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.14:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: White Collar
(n = 2545)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	48.4%	51.6%	36.9%	5.4%	4.2%	5.1%
Employment Status							
Employed	71.4	70.4	72.4	72.2	68.8	76.4	74.1
Unemployed	11.9	12.7	11.1	11.1	11.6	9.4	12.2
Out of Labor Force	16.7	16.9	16.5	16.7	19.6	14.2	13.7
Weeks of Joblessness[a]							
< 1	13.0	10.4	15.4	14.4	9.5	21.0	23.7
1 - 4	21.3	23.0	19.6	20.6	18.3	19.1	14.5
5 - 12	18.9	21.4	16.8	17.0	21.2	16.2	10.7
13 - 26	13.5	12.8	14.1	14.5	11.7	14.3	13.7
27 - 52	19.1	18.5	19.8	19.6	22.6	18.1	19.1
> 52	14.2	13.9	14.4	13.8	16.8	11.4	18.3
Censored	20.5	20.1	20.8	20.4	24.1	17.1	22.9
Wage Ratio[b]							
< 0.75	28.8	27.9	29.6	30.3	30.4	25.8	27.4
0.75 - 0.9	12.1	12.0	12.2	10.5	15.2	16.7	16.7
0.9 - 1.1	24.4	24.9	23.9	23.5	22.8	28.8	23.8
1.1 - 1.25	10.2	8.9	11.3	11.3	7.6	12.1	14.3
> 1.25	24.6	26.3	23.0	24.4	24.1	16.7	17.9

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Table 2.15:
Employment Status, Weeks of Joblessness, and Wage Changes by Type of Advance Notice: Blue Collar
(n = 1513)

	All Workers	No Notice	Received Notice	Type of Notice			
				Unwritten	Written (# months)		
					≤1	1-2	≥2
Total	100.0%	44.7%	55.3%	39.5%	7.3%	3.8%	4.6%
Employment Status							
Employed	76.1	73.4	78.4	78.8	77.5	84.2	71.4
Unemployed	21.2	23.6	19.3	18.9	20.7	10.5	27.1
Out of Labor Force	2.6	3.0	2.4	2.3	1.8	5.3	1.4
Weeks of Joblessness[a]							
< 1	10.1	8.2	11.7	12.5	8.2	7.0	14.3
1 - 4	23.1	23.0	23.2	23.6	26.4	15.8	21.4
5 - 12	16.8	17.6	16.2	16.7	15.5	14.0	14.3
13 - 26	16.2	15.8	16.5	16.7	16.4	21.1	11.4
27 - 52	23.1	23.0	23.2	21.7	23.6	29.8	30.0
> 52	10.6	12.4	9.2	8.9	10.0	12.3	8.6
Censored	17.4	19.3	15.8	15.7	15.5	15.8	17.1
Wage Ratio[b]							
< 0.75	30.5	34.9	27.1	26.5	24.7	40.5	23.9
0.75 - 0.9	13.2	11.9	14.2	13.2	26.0	7.1	8.7
0.9 - 1.1	22.1	21.2	22.8	23.4	14.3	33.3	21.7
1.1 - 1.25	12.5	11.2	13.4	14.5	6.5	7.1	21.7
> 1.25	21.9	20.9	22.6	22.4	28.6	11.9	23.9

[a] Weeks of Joblessness calculated as 28 weeks for persons displaced in 1987 and with right censored observations.

[b] Wage Ratio = Post-Displacement Wage/Pre-displacement wage. Ratio calculated for reemployed workers only.

Chapter 3:

Determinants of Advance Notice

In this chapter, we use binary and ordered probit methods to investigate which worker, firm, and geographic characteristics are associated with high probabilities of receiving various types of advance notice. This analysis (and the methods used throughout this study) are essentially descriptive in nature, given that no structural model of the advance notification process is constructed. As such, the findings may suggest which factors "cause" advance notice, but can not definitively ascertain causation. The more thorough understanding of who receives advance notice provided by this research, however, is an essential prerequisite to building a structural model. The role of endogenously provided advance notice is explicitly studied in chapter 6. This represents an important further step towards a more comprehensive framework in which causation can be determined.

We begin by briefly describing the probit and ordered probit estimation techniques used in this chapter. A latent variable representing the propensity for receiving advance notice is defined by:

$$N^* = Z\beta + \mu, \tag{3.1}$$

where N* is the latent variable, Z a vector of regression covariates, and μ an error term which is distributed N(0,1). In the binary model, the dummy variable N is equal to 1 if the requisite type of advance notice is obtained and N equals 0 if it is not. The nonrestrictive normalization

$$N = \begin{matrix} 1 \\ 0 \end{matrix} \quad \text{if} \quad N^* \begin{matrix} > \\ \le \end{matrix} 0\ , \tag{3.2}$$

is typically used.

This implies that

$$(3.3) \qquad N = \begin{matrix}1\\ \\0\end{matrix} \quad \text{if} \quad \mu \begin{matrix}>\\ \\ \le\end{matrix} -Z\beta.$$

With individual data, this model is estimated using maximum likelihood methods. The probability that N equals 1 [0] is F(.) [1 - F(.)], for F(.) the c.d.f. of the standard normal distribution evaluated at $Z\beta$. The log-likelihood function is therefore:

$$(3.4) \qquad L(\beta) = \sum_{N_i=0} \ln[1 - F(.)] + \sum_{N_i=1} \ln F(.)$$

and positive β coefficients imply increased probabilities of receiving notice.

The ordered probit case is only slightly more complicated. Consider the situation with three discrete outcomes which have a natural ordering (i.e. no written notice, written notice of less than k months, written notice of greater than k months). The same latent variable framework as in (3.1) is employed, however in this case, observed outcomes are:

$$(3.5) \qquad N = \begin{matrix}2\\1\\0\end{matrix} \quad \text{if} \quad \begin{matrix}N^* \ge k\\ 0 < N^* < k\\ N^* \le 0\end{matrix} \quad \text{or} \quad \begin{matrix}\mu \ge k - Z\beta\\ k - Z\beta > \mu > -Z\beta\\ \mu \le -Z\beta\end{matrix}.$$

Maximum likelihood is again used to estimate the regression coefficients.

3.1 Full Sample Results

Probit and ordered probit results on the probability of receiving various types of advance notice are presented below. In order to insure that the estimated coefficients are not sensitive to the particular set of included independent variables, preliminary regressions were estimated using a variety of covariates. Results of these runs are summarized in Tables 3.1 and 3.2.

The dependent variable EXPECT indicates whether respondents received notice of or otherwise expected their displacements (see Table 3.1). In Table 3.2, WRITTEN, shows whether respondents were notified of the job termination in writing. The tables show coefficient signs for covariates in each of 8 specifications and indicate ranges of values for the associated t statistics. A blank space indicates that the covariate was excluded from the regression in question. In model 1, the only regressors are personal characteristics (age, education, sex, etc.). Subsequent models include more comprehensive controls, with model 8 containing a total of 165 independent variables including a complete set of industry, occupation, state, and year dummy variables, personal traits, individual/firm characteristics, and area unemployment rates.

The tables reveal a striking stability of the coefficient estimates across regression regimes. There is only one where a variable which is statistically significant in one regression takes the opposite sign in another.[1] In addition to stability of the coefficient signs, the t statistics are generally similar across models. For instance, the regression coefficient is more than twice the standard error for health insurance coverage and for the plant closing dummy variable in all 8 models. This implies that there is considerable flexibility in the choice of supplemental regressors.

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[1] The exception is job tenure in Table 3.1. The coefficient on this variable is significantly positive in model 1 but becomes statistically insignificant when hours of work, the reason for displacement, and health insurance coverage are controlled for. In one regression (model 4) the coefficient is insignificantly negative.

The remainder of this chapter describes estimates of model 3. This represents a compromise between parsimony and completeness. Covariates are included for individual traits, person/firm characteristics, as well summary measures of industry, occupation, and geographic factors. The full set of industry, occupation, area, and year dummy variables is excluded, since these add little predictive power while dramatically reducing degrees of freedom.

Table 3.3 presents full sample estimates of the binary and ordered probit models. EXPECT and WRITTEN have already been defined. WRITGT1 (WRITGT2) are dependent variables equal to 1 if written notice of greater than 1 month (2 months) is received and to 0 otherwise. ADVTIME, the dependent variable in the ordered probit model, is equals 0 in the absence of written notice and is set to 1, 2, and 3, respectively, for formal notice of <1, 1 to 2, and >2 months duration.[2]

Individuals covered by group health insurance, married workers, females, and especially workers involved in plant closings are more likely than their counterparts to have anticipated their displacements (see column 1). The same is true for persons displaced from highly unionized industries or occupations, and for those residing in states with mandatory advance notice regulations. On the other hand, voluntary programs encouraging advance notice have no statistically discernible effect and there is no evidence that state unionization rates, right-to-work laws, or the area unemployment rate affect advance notice propensities.

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[2] Because of the high multicollinearity, the ordered probit model did not converge when the full set of covariates was included. Some independent variables were therefore excluded from the ordered probit estimates.

These results are largely consistent with the descriptive findings in Chapter 2, although the importance of race, industry growth rates, and occupation unemployment rates decline once other factors are controlled for. The small and insignificant coefficient on the earnings residual also confirms Ehrenberg & Jakubson (1988) finding that notified workers do not "pay" for advance notice by receiving a negative compensating wage differential on the predisplacement job.[3]

Factors which raising the probability that displacements are expected also generally increase the propensities of receiving formal advance notice. Thus, written notice and lengthy prenotification is more common for females, workers involved in plant closings or covered by group health insurance plans, highly unionized occupations, and states with mandatory advance notice provisions (see columns 2 through 5). Preseparation employment in a highly unionized industries raises the probability of receiving short but not lengthy durations of written notice.

Increasing seniority raises the probability of being formally notified far more than it increases the more general awareness of impending displacements. Persons obtaining written notice receive significantly higher wages than their counterparts. This means that workers not only do not "pay" for formal notice, they are actually receive a wage premium prior to obtaining it. The union effect is also larger for written than general notification and members of multiple earner households have high relative probabilities of being formally warned. Finally, slack local labor markets or high occupation unemployment rates reduce the probability of obtaining written notice.

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[3] Ehrenberg & Jakubson also indicate the positive effect of health insurance coverage and the industry unionization rate on advance notice probabilities. They do not examine whether there is a separate plant closing effect.

3.2 Demographic Group Differences

Tables 3.4 through 3.6 display coefficient estimates for the probit and ordered probit regressions on subsamples stratified by gender and reason for displacement. The positive effect of health insurance coverage on notification probabilities is uniformly large for all types of advance notice and across subsamples. Displacement due to plant closings, however, raises the likelihood of prenotification much more for males than females.

There are a number of other important demographic group differences. Education increases the probability of receiving substantial written notice for males and laidoff workers but not for those involved in plant closings or females. Married women and laidoff workers are less likely than their counterparts to be receive early warning (particularly formal notice) but members of these groups with positive preseparation wage residuals obtain written announcements relatively frequently.

Displacement due to plant closings raises probabilities more for males than females, as does being the head of household. The number of children, however, is positively related to written notice probabilities for females but not males. Blacks appear less likely than whites to be notified prior to layoffs but the reverse may be true before plant closings.

Interestingly, the impact of mandatory state regulations is concentrated on workers involved in partial layoffs. This probably occurs because firms closing plants are more likely to notify their employees in the absence of legislated requirements. Similarly, written notification becomes more likely prior to layoffs, but not plant closures, as the industry unionization and employment growth rate increases.

3.3 Summary and Implications

Although a full understanding of the determinants of advance notice must await the development of a comprehensive model of layoffs, the results in this chapter are informative. Some are expected, such as that unionized and high seniority workers receive notice more often than other groups. Others are less easy to explain. For example, it is not clear why displaced workers in multiple earner families are prenotified more frequently than those providing sole financial support.

One of the most interesting findings is that workers do not pay for advance notice by receiving a negative compensating wage differential. To the contrary, desirable working conditions and advance notice often go together. Prenotified individuals are relatively likely to have worked in jobs providing group health insurance coverage and those with written advance notice actually received positive wage residuals in their former positions.

A number of factors (i.e. state level regulations, education, and high industry unionization or employment growth rates) raise advance notice propensities prior to partial layoffs but not plant closings. This, combined with more frequent notice for workers involved in plant shutdowns, suggests that prenotification is more often viewed as a choice decision by employers in the case of mass layoffs than for plant closings. It also implies that the recently approved federal legislation is likely to increase notification rates more for the former group than for the latter.

Finally, there are important gender differences in advance notice propensities. As with male/female differentials in wages and working conditions, a full explanation of these disparities is likely to be difficult to obtain but probably includes some combination of choice and discrimination.

Table 3.1: Summary Results of Probit Regressions
Dependent Variable = EXPECT (n = 3577)

Regressor	Model							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EXP	+	+	+	+	+	+	+	+
EXPSQ	−	−	−	$-^{1}$	−	$-^{1}$	−	$-^{1}$
EDUC	+	+	+	+	+	+	−	−
EDUCSQ	−	−	−	+	−	−	−	+
TENURE	$+^{4}$	+	+	−	+	+	+	+
TENURESQ	−	+	+	$+^{1}$	+	+	+	+
MARRIED	$+^{2}$	$+^{3}$	$+^{3}$	$+^{3}$	$+^{3}$	$+^{3}$	$+^{3}$	$+^{2}$
HEAD	−	−	−	−	−	−	−	−
FEMALE	$+^{1}$	$+^{1}$	$+^{3}$	$+^{3}$	$+^{4}$	$+^{1}$	$+^{2}$	$+^{3}$
BLACK	$-^{1}$	−	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$	−	−
OTHR RACE	−	−	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$
NUMCHILD	−	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$	$-^{1}$
NUMEARN	$+^{1}$	+	+	+	+	+	+	+
PRTPREV		$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{2}$
HLTH INS		$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$
CLOSING		$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$	$+^{4}$
RESID		−	−	−	−	−	−	−
STEUNION			+	+	+	+	+	
RTWORK			+	+	+	+	+	
URATE			+	−	−			
AREARATE			−	$-^{1}$	−	−	−	−
INDCHG			$+^{1}$	+	$+^{1}$			
OCCRATE			+	+	+			
INDUNION			$+^{4}$	$+^{4}$	$+^{4}$			
OCCUNION			$+^{3}$	$+^{3}$	$+^{2}$			
MANDSTE			$+^{3}$	$+^{3}$	$+^{3}$	$+^{2}$	$+^{2}$	
VOLSTE			+	+	+	+	+	
UIPROB				+				
# Ind Dummies	0	0	0	0	2	19	47	47
# Occ "	0	0	0	0	2	12	44	44
# Year "	0	0	0	0	0	4	4	4
# State "	0	0	0	0	0	0	0	50
−2L	57.75	154.79	184.30	184.97	187.52	253.77	277.42	321.01
D.F.	13	17	27	28	31	58	119	165

[1] T statistic is greater than 1.0

[2] T statistic is greater than 1.5

[3] T statistic is greater than 1.75

[4] T statistic is greater than 2.0

Table 3.2: Summary Results of Probit Regressions
Dependent Variable = WRITTEN (n = 3577)

Regressor	Model (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EXP	−[2]	−[1]	−[1]	−	−[1]	−[1]	−[1]	−[1]
EXPSQ	+	+	+	+	+	+	+	+
EDUC	−	−	−	−	−	+	−	−
EDUCSQ	+	+	+	+	+	−	+	+
TENURE	+[4]	+[4]	+[4]	+[4]	+[4]	+[4]	+[4]	+[4]
TENURESQ	−[4]	−[4]	−[4]	−	−[4]	−[4]	−[4]	−[4]
MARRIED	−[2]	−[2]	−[2]	−[1]	−[2]	−[1]	−[2]	−[1]
HEAD	+[1]	+[1]	+[2]	+[2]	+[2]	+[2]	+[1]	+[2]
FEMALE	+[3]	+[4]	+[4]	+[4]	+[4]	+[2]	+[3]	+[3]
BLACK	+	+	+	+	+	+	+	−
OTHR RACE	+[4]	+[4]	+[3]	+[4]	+[3]	+[3]	+[4]	+[2]
NUMCHILD	+	+	+[1]	+[1]	+	+	+	+
NUMEARN	+[4]	+[3]	+[4]	+[3]	+[4]	+[3]	+[3]	+[3]
PRTPREV		+	+	+[3]	+	+	−	+
HLTH INS		+[4]	+[4]	+[4]	+[4]	+[4]	+[4]	+[4]
CLOSING		+[4]	+[4]	+[4]	+[4]	+[4]	+[4]	+[4]
RESID		+[4]	+[4]	+[2]	+[4]	+[4]	+[4]	+[4]
STEUNION			+[1]	+[1]	+[1]	+[2]	+[3]	
RTWORK			−[2]	−[1]	−[2]	−[1]	−[2]	
URATE			−	−	−			
AREARATE			−[1]	−[1]	−[1]	−[1]	−[3]	−
INDCHG			+[2]	+[3]	+[2]			
OCCRATE			+	+	+			
INDUNION			+[4]	+[4]	+[4]			
OCCUNION			+[2]	+[2]	+[2]			
MANDSTE			+[1]	+[2]	+[1]	+[1]	+[1]	
VOLSTE			+	+	+	+	+	
UIPROB				+[4]				
# Ind Dummies	0	0	0	0	2	19	47	47
# Occ "	0	0	0	0	2	12	44	44
# Year "	0	0	0	0	0	4	4	4
# State "	0	0	0	0	0	0	0	50
-2L	100.72	159.70	190.07	195.08	190.69	289.24	321.53	372.30
D.F.	13	17	27	28	31	58	119	165

[1] T statistic is greater than 1.0

[2] T statistic is greater than 1.5

[3] T statistic is greater than 1.75

[4] T statistic is greater than 2.0

Table 3.3:
Advance Notice Probit Regressions: All Workers

Regressor	Binary Probit Estimates				Ordered Probit Estimates
	Expect	Written	Writgt1	Writgt2	
EXP	5.5E-3	-0.016	-0.018	-0.018	-8.6E-3
	(0.57)	(1.33)	(1.31)	(1.06)	(2.65)
EXPSQ	-1.9E-4	1.7E-4	2.2E-4	2.1E-4	
	(0.88)	(0.65)	(0.71)	(0.55)	
EDUC	9.0E-3	-0.014	-0.118	-0.155	0.021
	(0.17)	(0.22)	(1.64)	(1.77)	(1.64)
EDUCSQ	-1.3E-4	1.6E-3	5.6E-3	6.9E-3	
	(0.06)	(0.64)	(2.00)	(2.02)	
TENURE	3.2E-3	0.044	0.062	0.059	0.024
	(0.33)	(3.99)	(4.80)	(3.78)	(5.41)
TENSQ	2.4E-4	-8.3E-4	-1.2E-3	-9.4E-4	
	(0.71)	(2.16)	(2.68)	(1.85)	
MARRIED	0.107	-0.113	-0.150	-0.168	-0.098
	(1.76)	(1.51)	(1.69)	(1.55)	(1.31)
HEAD	-0.038	0.123	0.089	-0.036	0.118
	(0.64)	(1.64)	(1.00)	(0.32)	(1.55)
FEMALE	0.112	0.207	0.171	0.155	0.177
	(1.91)	(2.83)	(1.97)	(1.41)	(2.42)
BLACK	-0.103	0.043	0.061	0.029	0.034
	(1.40)	(0.48)	(0.57)	(0.20)	(0.38)
OTHRACE	-0.182	0.281	0.190	0.189	0.212
	(1.43)	(1.97)	(1.14)	(0.95)	(1.46)
NUMCHILD	-0.021	0.024	0.022	4.4E-3	0.015
	(1.06)	(1.02)	(0.78)	(0.12)	(0.63)
NUMEARN	0.020	0.070	0.097	0.150	0.083
	(0.78)	(2.28)	(2.69)	(3.48)	(2.74)
PRTPREV	0.212	0.108	0.157	0.070	0.095
	(2.46)	(0.94)	(1.12)	(0.35)	(0.83)
HINS	0.331	0.385	0.369	0.519	0.426
	(6.05)	(5.36)	(4.15)	(4.14)	(6.07)
CLOSING	0.355	0.167	0.408	0.453	0.223
	(8.05)	(3.07)	(6.14)	(5.30)	(3.95)
RESID	-0.033	0.137	0.165	0.112	0.131
	(0.66)	(2.16)	(2.16)	(1.14)	(2.03)
STEUNION	3.7E-3	6.7E-3	7.1E-3	0.018	8.8E-3
	(0.95)	(1.41)	(1.27)	(2.52)	(1.91)
RTWORK	0.044	-0.121	-0.246	-0.148	-0.129
	(0.71)	(1.54)	(2.61)	(1.21)	(1.68)
URATE	1.0E-3	-3.9E-3	-4.5E-3	7.2E-3	2.9E-3
	(0.04)	(0.14)	(0.14)	(0.17)	(0.11)
AREARATE	-9.3E-3	-0.016	-0.017	-0.042	-0.023
	(0.87)	(1.19)	(1.08)	(2.13)	(1.73)
INDCHG	0.015	0.023	2.0E-3	-0.022	-4.8E-3
	(1.29)	(1.58)	(0.11)	(1.00)	(0.46)

OCCRATE	8.5E-3	9.2E-3	-0.031	-0.057	0.010
	(0.84)	(0.73)	(2.05)	(2.74)	(0.99)
INDUNION	6.0E-3	6.1E-3	-1.6E-3	-3.2E-3	
	(2.55)	(2.12)	(0.46)	(0.73)	
OCCUNION	4.0E-3	4.5E-3	5.8E-3	9.0E-3	
	(1.77)	(1.57)	(1.69)	(2.02)	
MANDSTE	0.251	0.219	0.243	0.253	0.251
	(1.81)	(1.43)	(1.41)	(1.22)	(1.71)
VOLSTE	0.073	0.058	1.8E-3	0.094	0.064
	(0.88)	(0.62)	(0.02)	(0.75)	(0.71)
Dep. Mean	0.530	0.160	0.094	0.051	
$\hat{\beta}\bar{X}/\sigma$	0.078	-1.060	-1.456	-1.892	
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Regressions estimated by maximum likelihood probit. Absolute values of asymptotic T statistics in parentheses.

2. Binary Probit Dependent Variables (equal one if):

   Expect: expected displacement (whether notice was formal or informal).
   Written: written advance notice was received.
   Writgt1: more than one month's written advance notice received.
   Writgt2: more than two month's written advance notice received.

3. Ordered Probit Dependent Variable:

   Advtime = 0 if no written advance notice received;
   = 1 if less than one month written advance notice received;
   = 2 if between one and two months written advance notice received;
   = 3 if greater than two months written advance notice received.

4. Some variables are excluded from the ordered probit model because of their high collinearity with other included variables.

Table 3.4:
Advance Notice Binary Probit Regressions:
Dependent Variable = EXPECT

Regressor	Males	Females	Plant Closings	Layoffs
EXP	-2.7E-3	0.016	0.019	-4.5E-3
	(0.21)	(1.14)	(1.38)	(0.32)
EXPSQ	-9.3E-5	-3.3E-4	-4.2E-4	-3.6E-5
	(0.32)	(1.03)	(1.40)	(0.11)
EDUC	0.033	-0.039	6.0E-3	0.019
	(0.50)	(0.45)	(0.09)	(0.20)
EDUCSQ	-8.7E-4	1.6E-3	-1.7E-4	-2.4E-4
	(0.34)	(0.48)	(0.07)	(0.07)
TENURE	2.7E-3	5.2E-3	6.7E-3	-2.2E-3
	(0.22)	(0.30)	(0.53)	(0.15)
TENSQ	2.8E-4	3.2E-4	9.1E-5	5.7E-4
	(0.68)	(0.41)	(0.21)	(0.96)
MARRIED	0.213	0.166	0.193	0.025
	(2.12)	(1.32)	(2.28)	(0.28)
HEAD	-0.118	0.077	3.0E-3	-0.103
	(1.07)	(0.61)	(0.04)	(1.17)
FEMALE			0.123	0.075
			(1.51)	(0.87)
BLACK	-0.078	-0.159	0.010	-0.251
	(0.75)	(1.48)	(0.10)	(2.28)
OTHRACE	-0.162	-0.209	-0.091	-0.298
	(0.95)	(1.07)	(0.52)	(1.57)
NUMCHILD	-0.045	-1.5E-4	-0.053	0.012
	(1.69)	(0.01)	(1.95)	(0.41)
NUMEARN	-2.3E-3	0.035	7.5E-3	0.043
	(0.07)	(0.90)	(0.21)	(1.16)
PRTPREV	0.284	0.218	0.228	0.183
	(1.73)	(2.01)	(1.86)	(1.47)
HINS	0.361	0.305	0.372	0.303
	(4.77)	(3.78)	(4.89)	(3.78)
CLOSING	0.323	0.411		
	(5.50)	(6.03)		
RESID	-0.057	0.042	-0.090	0.021
	(0.86)	(0.52)	(1.28)	(0.28)
STEUNION	-2.7E-3	0.012	1.7E-3	5.9E-3
	(0.51)	(2.01)	(0.31)	(1.07)
RTWORK	-0.028	0.131	0.051	0.012
	(0.33)	(1.37)	(0.57)	(0.13)
URATE	0.019	-0.016	0.027	-0.024
	(0.63)	(0.43)	(0.86)	(0.74)

AREARATE	-0.039	0.034	-0.035	0.019
	(2.75)	(2.02)	(2.34)	(1.21)
INDCHG	3.4E-3	0.038	-1.0E-3	0.040
	(0.24)	(1.88)	(0.06)	(2.45)
OCCRATE	0.013	1.2E-3	3.4E-3	0.011
	(1.03)	(0.07)	(0.23)	(0.76)
INDUNION	4.3E-3	0.010	-1.8E-3	0.016
	(1.42)	(2.56)	(-0.55)	(4.44)
OCCUNION	4.7E-3	4.2E-3	1.1E-3	8.4E-3
	(1.64)	(1.07)	(0.35)	(2.53)
MANDSTE	0.032	0.539	0.017	0.440
	(0.17)	(2.51)	(0.09)	(2.18)
VOLSTE	0.102	0.043	0.121	0.014
	(0.93)	(0.34)	(1.08)	(0.11)
Dep. Mean	0.526	0.535	0.594	0.459
$\hat{\beta}\bar{X}/\sigma$	0.069	0.093	0.245	-0.109

See Table 3.3 for descriptive information.

Table 3.5:
Advance Notice Binary Probit Regressions:
Dependent Variable = WRITTEN

Regressor	Males	Females	Plant Closings	Layoffs
EXP	-0.026	-4.0E-3	-0.024	1.8E-4
	(1.58)	(0.22)	(1.46)	(0.01)
EXPSQ	4.1E-4	-6.2E-5	4.1E-4	-3.1E-4
	(1.11)	(0.16)	(1.18)	(0.71)
EDUC	0.047	-0.095	-0.021	0.110
	(0.52)	(0.98)	(-0.27)	(0.71)
EDUCSQ	-2.0E-4	4.0E-3	1.8E-3	-2.6E-3
	(0.06)	(1.06)	(0.58)	(0.46)
TENURE	0.057	0.026	0.052	0.032
	(3.93)	(1.31)	(3.57)	(1.78)
TENSQ	-1.2E-3	-2.1E-4	-1.1E-3	-2.5E-4
	(2.62)	(0.25)	(2.38)	(0.36)
MARRIED	-0.037	-0.332	-0.077	-0.160
	(0.30)	(2.23)	(0.76)	(1.42)
HEAD	0.257	-0.081	0.111	0.139
	(1.77)	(0.54)	(1.10)	(1.23)
FEMALE			0.146	0.267
			(1.48)	(2.43)
BLACK	-0.015	0.071	0.144	-0.112
	(0.11)	(0.56)	(1.22)	(0.79)
OTHRACE	0.349	0.184	0.298	0.247
	(1.87)	(0.82)	(1.52)	(1.15)
NUMCHILD	-0.018	0.077	0.028	0.015
	(0.55)	(2.13)	(0.87)	(0.41)
NUMEARN	0.050	0.089	0.034	0.126
	(1.14)	(1.89)	(0.81)	(2.73)
PRTPREV	0.144	0.212	-0.071	0.288
	(0.67)	(1.46)	(0.43)	(1.74)
HINS	0.284	0.487	0.424	0.339
	(2.79)	(4.71)	(4.37)	(3.10)
CLOSING	0.200	0.131		
	(2.76)	(1.55)		
RESID	0.016	0.344	0.011	0.311
	(0.20)	(3.32)	(0.12)	(3.24)
STEUNION	2.3E-3	0.011	7.2E-3	7.5E-3
	(0.35)	(1.51)	(1.08)	(1.08)
RTWORK	-0.212	-0.011	-0.162	-0.058
	(2.01)	(0.09)	(1.50)	(0.49)
URATE	-0.025	0.021	7.7E-3	-9.4E-3
	(0.68)	(0.48)	(0.20)	(0.22)

AREARATE	-0.028	6.8E-3	-0.034	2.5E-3
	(1.57)	(0.33)	(1.90)	(0.12)
INDCHG	0.027	3.2E-3	-0.016	0.068
	(1.52)	(0.13)	(0.79)	(3.27)
OCCRATE	0.021	-0.014	4.6E-3	0.013
	(1.37)	(0.66)	(0.27)	(0.72)
INDUNION	8.4E-3	5.7E-4	-6.4E-3	0.021
	(2.33)	(0.12)	(1.60)	(4.83)
OCCUNION	4.1E-3	7.1E-3	6.4E-3	1.8E-3
	(1.13)	(1.46)	(1.65)	(0.41)
MANDSTE	0.239	0.242	0.049	0.367
	(1.14)	(1.05)	(0.22)	(1.69)
VOLSTE	0.028	0.125	0.220	-0.231
	(0.22)	(0.86)	(1.83)	(-1.43)
Dep. Mean	0.161	0.160	0.179	0.140
$\hat{\beta}\bar{X}/\sigma$	-1.064	-1.085	-0.988	-1.181

See Table 3.3 for descriptive information.

Table 3.6:
Advance Notice Ordered Probit Regressions:
Dependent Variable = ADVTIME

Regressor	Males	Females	Plant Closings	Layoffs
EXP	-8.7E-3	-6.1E-3	-6.9E-3	-0.012
	(1.95)	(1.19)	(1.55)	(2.27)
EDUC	0.031	5.5E-3	9.1E-3	0.046
	(1.86)	(0.27)	(0.52)	(2.25)
TENURE	0.025	0.023	0.021	0.031
	(4.54)	(2.70)	(3.67)	(3.95)
MARRIED	-0.021	-0.331	-0.043	-0.197
	(0.17)	(2.24)	(0.44)	(1.68)
HEAD	0.256	-0.087	0.130	0.115
	(1.81)	(0.59)	(1.26)	(1.00)
FEMALE			0.129	0.248
			(1.30)	(2.25)
BLACK	-0.010	0.053	0.124	-0.135
	(0.08)	(0.42)	(1.05)	(0.92)
OTHRACE	0.267	0.150	0.284	0.138
	(1.42)	(0.64)	(1.43)	(0.65)
NUMCHILD	-0.030	0.073	7.7E-3	0.024
	(0.83)	(1.94)	(0.23)	(0.63)
NUMEARN	0.072	0.091	0.067	0.120
	(1.65)	(1.97)	(1.59)	(2.63)
PRTPREV	0.125	0.187	-0.074	0.270
	(0.61)	(1.24)	(0.44)	(1.63)
HINS	0.383	0.465	0.498	0.313
	(3.78)	(4.48)	(5.12)	(2.92)
CLOSING	0.292	0.151		
	(3.78)	(1.74)		
RESID	-6.4E-3	0.347	1.2E-3	0.320
	(0.07)	(3.33)	(0.01)	(3.30)
STEUNION	7.4E-3	9.3E-3	9.8E-3	9.3E-3
	(1.18)	(1.32)	(1.53)	(1.37)
RTWORK	-0.172	-0.077	-0.166	-0.069
	(1.62)	(0.65)	(1.53)	(0.61)
URATE	-0.033	0.050	7.6E-3	-3.1E-4
	(0.93)	(1.20)	(0.22)	(0.01)
AREARATE	-0.030	-6.7E-3	-0.034	-7.0E-3
	(1.70)	(0.32)	(2.01)	(0.32)
INDCHG	-7.0E-3	-7.9E-3	2.5E-3	-9.1E-3
	(0.52)	(0.45)	(0.18)	(0.56)
OCCRATE	0.024	-0.013	9.1E-3	0.012
	(1.83)	(0.72)	(0.66)	(0.76)
MANDSTE	0.274	0.295	0.142	0.373
	(1.37)	(1.32)	(0.68)	(1.73)
VOLSTE	0.058	0.124	0.229	-0.295
	(0.46)	(0.89)	(2.00)	(1.78)

See Table 3.3 for descriptive information.

Chapter 4:

Advance Notice and Postdisplacement Joblessness

We are now ready to estimate reduced form models examining the relationship between advance notice and the duration of joblessness, the probability of avoiding nonemployment, and the likelihood of being employed in January 1988. Throughout this chapter and the next, we assume that advance notice is provided exogenously. Consideration of endogenous advance notice is reserved for Chapter 6.

The methodology and findings of earlier research, using the 1984 and 1986 DWS, are replicated by utilizing the broadest definition of advance notice (which includes both informal and written announcements). These regressions again indicate that prenotification is correlated with moderate reductions in joblessness, largely as the result of increased in the probabilities of avoiding time out-of-work altogether. Restricting consideration to written advance notice, however, dramatically changes the results. Prior notification is still associated with a small increase in the likelihood of completely avoiding joblessness but there is no longer any reduction in the average duration of nonemployment.

We consider a model where weeks of postdisplacement joblessness (W) depends on a vector of covariates (X), a dummy variable indicating exogenously provided advance notice (N), and a disturbance term (ϵ). W is positive if:

$$I^* = X \alpha_1 + N \alpha_2 + \epsilon > 0$$

and equals zero if:

(4.1) $$I^* = X\alpha_1 + N\alpha_2 + \epsilon \leq 0,$$

for I^* a latent variable where $W = f(I^*)$, $f(0) = 0$, and $f' > 0$. If ϵ is normally distributed, consistent estimates of the effect of advance notice on the probability avoiding joblessness ($\Pr[W = 0] = \Pr[I^* \leq 0]$) can be obtained from maximum likelihood probit regressions of:

(4.2) $$\text{AVOID} = X\beta_1 + N\beta_2 + \mu,$$

where AVOID is a dummy variable which equals 1 (0) if I^* is greater (less) than 0. Positive regression coefficients then imply increased probabilities of avoiding joblessness.

Weeks of postdisplacement joblessness (WKSOUT) are right-censored in the DWS for individuals nonemployed more than 99 weeks and for those continuously out-of-work from the time of job loss until the survey date. Duration models are well suited to deal with right-censoring and have frequently been used to estimate the determinants of nonemployment durations.

In this section, the accelerated failure time model:

(4.3) $$W = \exp(Z\beta)W_0^{\sigma}, \qquad \ln W = Z\beta + \sigma\mu,$$

is estimated. In equation (4.3), $Z\beta = X\beta_1 + N\beta_2$, μ is an error term with density $f(.)$, $W_0 = \exp(\mu)$, and σ is a scale parameter. The underlying survivor function for the ith individual is:

$$S_i(W) = S_0[W \exp(-Z_i\beta)]. \tag{4.4}$$

The log of the expected duration of joblessness is linear in the covariates and the effect of the latter are to rescale time. The error term is assumed to have a generalized gamma distribution. The generalized gamma is a two parameter distribution which nests the exponential, Weibull, lognormal, gamma, and chi-square distributions, among others.[1] The log-likelihood function for this model is specified by:

$$L = \sum_{i=1}^{n} c_i[\ln f(\mu_i;k) - \ln \sigma] + \sum_{i=1}^{n}(1-c_i)\ln F(\mu_i;k), \tag{4.5}$$

where k is the shape parameter of the generalized gamma and c_i equals 0 (1) for censored (uncensored) observations. To reduce the frequency of right-censoring and the probability that respondents return to their previous employers after January 1988, the sample for the duration regressions is limited to persons displaced between 1983 and 1985 (at least 2 years before the survey date).[2]

4.1 Avoiding Joblessness

Table 4.1 presents results of probit regressions, estimated by maximum likelihood, where the dichotomous dependent variable is equal to 1 for individuals who are out-of-work less than 1 week following the termination of their predisplacement job and with WRITTEN included as the notification regressor.[3] Before focusing on advance notice, we briefly discuss the impact of the other covariates.

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[1] The use of generalized gamma distributions in accelerated failure time models is discussed extensively by Addison & Portugal (1987b).

[2] This also implies that all right censored observations have nonemployment durations top-coded at 99 weeks.

The likelihood of moving directly into new positions, without intervening joblessness, is relatively low for females, nonwhites, non-head of households, those displaced in partial layoffs, and for individuals without group health insurance.[4] Workers with substantial seniority or receiving positive predisplacement wage residuals obtain immediate reemployment relatively often. This is somewhat surprising given that both groups are generally thought to experience special adjustment problems.[5] Displaced workers in multiple earner households also avoid nonemployment fairly often as do respondents in occupations with low unemployment rates. Conversely, joblessness is more likely for full-time workers, residents of right-to-work states, and persons eligible for unemployment insurance benefits. Coefficients on state and occupation unionization rates are never statistically significant, nor are those on the area unemployment rate.

Some of the variables have substantially different effects when the sample is stratified by sex or the reason for displacement. For instance, the health insurance effect is weaker for men and laidoff workers than for women or persons displaced by plant closings. This pattern is reversed for unemployment insurance eligibility. Notwithstanding these disparities, most coefficients have a fairly stable pattern across demographic subgroups. We now consider the impact of various types of advance notice.

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[3] Coefficients on the other regressors are virtually identical when other advance notice variables are substituted for WRITTEN.

[4] Some of these results have been obtained previously by Podgursky & Swaim (1987b) and Ehrenberg & Jakubson (1988) using the 1984 DWS. Relatively rapid reemployment following plant closings may result from the low associated probabilities of recall.

[5] For example, see Hamermesh (1989). However, Ruhm (1989c) offers an opposing view.

1 out of 8 displaced workers is reemployed within a week of the initial job termination. The first 6 rows of Table 4.2 display probit coefficients showing the effect of various types of advance notice on the probability of avoiding joblessness. The regressor in model 1 (NOTICE) equals 1 if the job loss is expected or notice is received. In model 2, WRITTEN indicates the receipt of written notification of any duration. For the third model, ADV0 equals 1 for workers receiving unwritten notice or expecting the displacement; ADV1, ADV2, and ADV3 indicate written announcements of less than 1 month, 1 to 2 months, and over 2 months respectively. The full set of covariates is also included in each model. Since probit coefficients are somewhat difficult to interpret, rows 7 through 13 show the predicted effects of receiving each type of notice, with the non-notification regressors evaluated at the sample means.

Receiving notice or expecting the job termination raises the likelihood of avoiding joblessness. The coefficient on NOTICE is statistically significant for all subgroups except women and the predicted increase, compared to non-notified workers, ranges from 1.9 to 6.2 percentage points (28.8 to 59.0 percent). The increment is larger for males than females and of approximately equal size for plant closings and partial layoffs (see Table 4.2: rows 7 and 8).

The above results replicate the findings of previous researchers. Estimates using narrower definitions of prior notification are new, however, and indicate that written advance notice has a much weaker impact for males and workers displaced by plant closings. The coefficient on WRITTEN is less than one-ninth the size of that on NOTICE for the former group and only one-third as large for the latter. Where informal notice (ADVO) is associated with a 69.5 percent increase in the probability that males avoid joblessness and a 44.9 percent rise for those affected by plant closings, the predicted impact of written notice is generally negative (see columns 2 and 4).

Formal notice, especially when received well before displacement, does substantially increase the ability of women and workers terminated during partial layoffs to avoid joblessness. The receipt of written notification raises the probability that these groups move directly into new employment by at least 50 percent; formal announcements exceeding 2 months increase the respective probabilities by 72.5 and more than 100 percent.

4.2 Postdisplacement Joblessness

We now consider whether, in addition to raising the likelihood of immediate reemployment, advance notice reduces the average duration of postdisplacement joblessness. Table 4.3 shows the full set of coefficients estimates for regressions where written advance notice is controlled for. Tables 4.4 and 4.5 follow by summarizing the impact of various types of advance notice.

Many of the covariates which were shown (in Table 4.2) to be associated with low probabilities of avoiding nonemployment also predict lengthy postdisplacement joblessness. Thus, the duration of spells is expected to be relatively long for women, minorities, non-heads of households, persons displaced during partial layoffs, and members of single earner households (see Table 4.3). Conversely, job tenure, the occupation unemployment rate, work hours, and predicted unemployment insurance eligibility have no effect on durations but do influence the probability of avoiding joblessness.

Respondents with large families, residing in local labor markets with high unemployment, or working in slow growing industries (prior to displacement) are jobless for longer than their counterpart, even though these factors do not appear to reduce the probability of avoiding nonemployment. Positive wage residuals similarly delay reemployment by much more than they reduce the probability of avoiding joblessness. Finally, right-to-work laws are associated with shorter periods out-of-work, despite the fact that they reduce the probability of avoiding all joblessness. Although some coefficients vary across demographic groups, important disparities continue to be the exception rather than the rule.[6]

Tables 4.4 and 4.5 display accelerated failure time estimates of the effect of various types of advance notice on postdisplacement joblessness. All respondents are included in the first table; table 4.5 is restricted to respondents with positive (≥1 week) durations of nonemployment.

Individuals expecting or receiving notice of job terminations are out-of work for less time than non-notified individuals. The coefficient on NOTICE is negative for all subgroups except females and, although measured imprecisely, is fairly large for males and laidoff respondents (see Table 4.4:

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[6] For example, the coefficient on the number of children is larger for women and those displaced in plant closings than for men or laidoff workers but the industry growth rate has a less important effect for females and workers displaced during partial layoffs.

row 1). Prenotified men are jobless approximately 15 percent (3.4 weeks) shorter than those receiving no warning; the reduction for laidoff individuals is 12.4 percent. This decrease occurs entirely because of increased propensities of notified individuals to avoid joblessness. There is no corresponding decrease in durations for those who experience more than 1 week of nonemployment. This can be seen by observing that when the sample is restricted to persons with some joblessness, the coefficient on NOTICE is positive for all subgroups and is almost statistically significant for females and respondents involved in plant closings (see Table 4.5, row 1).

There is no evidence that the receipt of written (as opposed to informal) notice is associated with any decrease in postdisplacement joblessness. The coefficients on WRITTEN and those on ADV1 through ADV3 are positive for all subgroups and approach statistical significance in some cases (Table 4.4: rows 2, 4, 5, and 6). Conversely, those on ADV0 are negative, large, and statistically significant (row 3).[7] Restricting the sample to persons jobless a week or more produces even larger positive (and generally statistically significant) coefficients for WRITTEN and ADV1 through ADV3 (see Table 4.5).

Workers notified in writing generally receive more detailed information about impending displacements than less formally notified individuals. It therefore does not appear plausible that the unwritten notice speeds reemployment while written warning retards it. A more probable explanation of the above findings is that the reduced form regression model is misspecified because it fails to account for the potential endogeneity of advance notice. This possibility is addressed in chapter 6.

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[7] Swaim & Podgursky (1989) and Portugal & Addison (1989) have recently argued that conventional accelerated failure time models understate the reduction in joblessness associated with advance notice because they fail to adequately account for predisplacement search. Given that written notice reduces the probability of avoiding nonemployment by less than informal announcements, the biases are less important for this analysis than for earlier work using the 1984 and 1986 DWS. Nonetheless, this question should be considered explicitly for written advance notice in future work.

4.3 Survey Date Employment

To examine the relationship between prior notification and longer term employment stability, probit regressions on the probability of working for pay in January 1988 were also estimated. 22 of the 30 resulting advance notice coefficients were positive, although none were statistically significant (see Table 4.6). This suggests that prior notification either has no effect on or slightly improves future employment prospects.

4.4 Summary

The findings of this chapter can be summarized as follows. First, workers anticipating or receiving notice of permanent job terminations are more likely to move directly into new employment than their non-notified counterparts. The impact is stronger for unwritten than written notification, however, and persons formally alerted less than 1 month before the permanent layoff have an unambiguously lower probability of avoiding joblessness. Second, the small reduction in the average duration of nonemployment, associated with receiving advance notice, conceals a much larger decrease for informally notified individuals, combined with a sizable increase for those receiving written announcements. Even for the former group, however, the decrease is entirely explained by their elevated probability of avoiding nonemployment. Notification makes no difference for displaced workers who failing to immediately obtain new positions.

The results suggest shortcomings in previous analyses where advance notice has included poorly defined "expectations" of impending displacements. To the extent that better informed workers are more likely to anticipate job loss, and also find new employment relatively rapidly, the beneficial impact of prior warning will be overestimated. This also occurs if respondents with unusually strong aversions to joblessness both forecast displacements and to prepare for them in such a way as to facilitate quick reemployment. The extended nonemployment of workers with written notice may also indicate significant endogeneity bias. Finally, the fairly large differences in the effects of advance notice across sample subgroups argue against assuming that prenotification changes only the regression intercept.

Table 4.1:
Probit Regressions Indicating the Effect of Written Advance Notice on the Probability of Avoiding Postdisplacement Joblessness

			Subsample		
Regressor	All Workers	Males	Females	Plant Closings	Layoffs
EXP	−0.019	−0.023	−0.012	1.9E-3	−0.047
	(1.43)	(1.28)	(0.54)	(0.10)	(2.31)
EXPSQ	2.3E-4	2.5E-4	1.7E-4	−2.9E-4	9.4E-4
	(0.75)	(0.63)	(0.32)	(0.70)	(1.95)
EDUC	8.0E-3	−0.090	0.612	0.074	−0.149
	(0.10)	(1.06)	(2.17)	(0.76)	(1.09)
EDUCSQ	1.0E-3	4.5E-3	−0.020	−1.8E-3	6.8E-3
	(0.34)	(1.37)	(1.96)	(0.46)	(1.36)
TENURE	0.038	0.059	0.026	0.039	0.031
	(2.41)	(3.09)	(0.78)	(1.89)	(1.24)
TENSQ	-1.1E-3	-1.5E-3	-2.1E-3	-1.2E-3	−6.8E-4
	(2.05)	(2.42)	(1.27)	(1.64)	(0.77)
MARRIED	0.075	0.099	0.028	0.142	−0.039
	(0.88)	(0.74)	(0.14)	(1.24)	(0.29)
HEAD	0.313	0.382	0.304	0.326	0.326
	(3.58)	(2.27)	(1.57)	(2.81)	(2.35)
FEMALE	−0.101			−0.021	−0.212
	(1.20)			(0.19)	(1.59)
BLACK	−0.366	−0.205	−0.685	−0.386	−0.374
	(3.08)	(1.37)	(3.15)	(2.48)	(1.94)
OTHRACE	−0.308	−0.422	−0.197	−0.259	−0.454
	(1.63)	(1.67)	(0.65)	(1.05)	(1.45)
NUMCHILD	−0.021	−0.026	−6.3E-3	−0.046	0.021
	(0.80)	(0.76)	(0.14)	(1.29)	(0.52)
NUMEARN	0.137	0.123	0.162	0.111	0.178
	(3.87)	(2.63)	(2.73)	(2.32)	(3.27)
PRTPREV	0.327	0.587	0.369	0.524	0.095
	(2.64)	(2.96)	(2.06)	(3.15)	(0.48)
HINS	0.155	0.060	0.269	0.199	0.108
	(2.01)	(0.59)	(2.15)	(1.96)	(0.88)
CLOSING	0.265	0.139	0.511		
	(4.33)	(1.80)	(4.81)		
RESID	0.130	0.120	0.202	0.172	0.082
	(1.88)	(1.37)	(1.66)	(1.87)	(0.76)
STEUNION	−2.9E-3	−0.010	8.5E-3	−0.014	0.013
	(0.55)	(1.54)	(1.00)	(2.03)	(1.62)
RTWORK	−0.166	−0.246	−0.051	−0.314	0.016
	(1.96)	(2.25)	(0.36)	(2.81)	(0.12)
URATE	−4.1E-3	0.017	−0.052	−0.038	0.052
	(0.13)	(0.45)	(0.95)	(0.92)	(1.09)
AREARATE	−0.011	−0.013	−8.3E-3	−0.017	−6.3E-3
	(0.73)	(0.68)	(0.33)	(0.89)	(0.27)

INDCHG	0.013	6.7E-3	0.030	0.033	-7.7E-3
	(0.81)	(0.36)	(1.06)	(1.56)	(0.33)
OCCRATE	-0.027	-0.015	-0.042	-0.032	-0.023
	(1.96)	(0.90)	(1.60)	(1.70)	(1.10)
INDUNION	-4.2E-3	-3.5E-3	-4.5E-3	-5.8E-3	-2.6E-3
	(1.31)	(0.88)	(0.82)	(1.39)	(0.53)
OCCUNION	-1.6E-3	-1.7E-3	-8.2E-4	2.3E-3	-5.6E-3
	(0.51)	(0.45)	(0.14)	(0.56)	(1.18)
UIPROB	-0.422	-0.639	6.7E-3	-0.196	-0.753
	(1.86)	(2.17)	(0.02)	(0.66)	(2.09)
WRITTEN	0.125	0.030	0.243	0.060	0.234
	(1.64)	(0.30)	(1.98)	(0.61)	(1.90)
Dep. Mean	0.116	0.131	0.097	0.139	0.093
$\hat{\beta}\bar{X}/\sigma$	-1.282	-1.236	-1.506	-1.162	-1.418
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Regressions estimated by maximum likelihood probit. Absolute values of asymptotic T statistics in parentheses.
2. Dependent Variable equals one if worker is out of work less than one week following initial displacement and zero if he/she is jobless for one week or more.

Table 4.2: The Effect of Various Types of Advance Notice on the Probability of Avoiding Postdisplacement Joblessness

Adv. Notice Regressor	All Workers	Males	Females	Plant Closings	Layoffs
	Regression Cofficients:				
model one					
NOTICE	0.231	0.287	0.137	0.216	0.244
	(3.88)	(3.74)	(1.38)	(2.70)	(2.64)
model two					
WRITTEN	0.125	0.030	0.243	0.060	0.234
	(1.64)	(0.30)	(1.98)	(0.61)	(1.90)
model three					
ADV0	0.234	0.330	0.072	0.236	0.207
	(3.64)	(4.02)	(0.66)	(2.76)	(2.04)
ADV1	-0.202	-0.234	-0.206	-0.243	-0.150
	(1.50)	(1.42)	(0.84)	(1.24)	(0.79)
ADV2	0.043	-0.114	0.280	-0.086	0.359
	(0.32)	(0.64)	(1.36)	(0.51)	(1.61)
ADV3	0.126	-0.098	0.413	0.045	0.320
	(1.04)	(0.59)	(2.16)	(0.31)	(1.40)
Predicted Probability of Avoiding Joblessness					
NO NOTICE	0.099	0.105	0.066	0.118	0.080
NOTICE = 1	0.145	0.167	0.085	0.166	0.122
WRITTEN = 1	0.122	0.110	0.103	0.130	0.120
ADV0 = 1	0.146	0.178	0.076	0.171	0.115
ADV1 = 1	0.068	0.069	0.043	0.077	0.060
ADV2 = 1	0.105	0.086	0.109	0.102	0.147
ADV3 = 1	0.122	0.088	0.137	0.127	0.138
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Description of Advance Notice Dummy Variables. (Variable is equal to one if:)
   - NOTICE - "Expected" or received advance notice of job termination
   - WRITTEN - Received written advance notice of job termination
   - ADV0 - No written notice but received unwritten notice or expected job termination
   - ADV1 - Received less than one month's written advance notice
   - ADV2 - Received between one and two months written advance notice
   - ADV3 - Received more than two months written advance notice

2. Regressions estimated by maximum likelihood probit and include the full set of demographic, industry, occupation, and geographic covariates. Absolute values of asymptotic T statistics in parentheses.

3. Predicted probabilities calculated at the mean value of the non-notification explanatory variables.

Table 4.3:
Accelerated Failure Time Model of Written Notice and Postdisplacement Joblessness

	Subsample				
Regressor	All Workers	Males	Females	Plant Closings	Layoffs
EXP	0.026	-0.017	0.075	0.017	0.037
	(1.35)	(0.69)	(2.47)	(0.66)	(1.27)
EXPSQ	6.6E-5	0.001	-0.001	3.3E-4	-3.1E-4
	(0.16)	(1.86)	(1.40)	(0.58)	(0.46)
EDUC	0.003	0.157	-0.251	0.006	0.020
	(0.03)	(1.21)	(1.34)	(0.05)	(0.11)
EDUCSQ	-0.001	-0.006	0.006	-0.001	-0.002
	(0.32)	(1.25)	(0.90)	(0.21)	(0.31)
TENURE	0.018	0.024	-0.052	-0.005	0.056
	(0.83)	(0.94)	(1.18)	(0.16)	(1.65)
TENSQ	5.1E-4	-3.5E-5	0.005	0.001	-3.3E-4
	(0.68)	(0.04)	(2.50)	(1.04)	(0.25)
MARRIED	0.037	-0.094	0.182	0.057	0.003
	(0.31)	(0.54)	(0.66)	(0.35)	(0.02)
HEAD	-0.731	-0.441	-0.768	-0.776	-0.629
	(6.14)	(2.17)	(2.82)	(4.83)	(3.60)
FEMALE	0.396			0.294	0.517
	(3.50)			(1.93)	(3.08)
BLACK	0.639	0.549	0.710	0.832	0.310
	(4.38)	(3.01)	(2.95)	(4.12)	(1.45)
OTHRACE	1.002	1.350	0.545	0.864	1.168
	(4.16)	(4.42)	(1.40)	(2.74)	(3.11)
NUMCHILD	0.161	0.080	0.274	0.210	0.093
	(4.25)	(1.77)	(4.38)	(4.16)	(1.77)
NUMEARN	-0.363	-0.277	-0.462	-0.381	-0.344
	(7.12)	(4.37)	(5.26)	(5.38)	(4.68)
PRTPREV	0.097	0.258	-0.179	-0.114	0.263
	(0.51)	(0.77)	(0.69)	(0.43)	(0.94)
HINS	-0.109	0.081	-0.318	-0.032	-0.306
	(1.04)	(0.60)	(1.94)	(0.22)	(1.99)
CLOSING	-0.268	-0.088	-0.556		
	(3.12)	(0.84)	(3.81)		
RESID	-0.122	-0.162	-0.079	-0.283	0.094
	(1.24)	(1.38)	(0.45)	(2.06)	(0.67)
STEUNION	-0.010	-0.011	-0.006	-2.7E-4	-0.021
	(1.28)	(1.16)	(0.51)	(0.02)	(1.92)
RTWORK	-0.262	-0.309	-0.098	-0.102	-0.402
	(2.20)	(2.08)	(0.51)	(0.61)	(2.43)
URATE	-0.020	-0.013	-0.043	0.028	-0.076
	(0.45)	(0.24)	(0.57)	(0.46)	(1.19)
AREARATE	0.104	0.105	0.094	0.105	0.100
	(4.95)	(4.03)	(2.59)	(3.66)	(3.17)

INDCHG	-0.051	-0.061	-0.044	-0.085	-0.025
	(2.32)	(2.39)	(1.07)	(2.72)	(0.80)
OCCRATE	0.008	0.001	-0.004	0.003	0.023
	(0.42)	(0.06)	(0.10)	(0.11)	(0.87)
INDUNION	-0.002	-0.007	0.002	-0.002	-0.005
	(0.44)	(1.33)	(0.26)	(0.38)	(0.69)
OCCUNION	0.003	0.002	0.007	0.003	-0.002
	(0.70)	(0.49)	(0.82)	(0.43)	(0.28)
UIPROB	0.165	0.627	-0.307	0.100	0.260
	(0.49)	(1.49)	(0.56)	(0.22)	(0.51)
WRITTEN	0.186	0.086	0.313	0.129	0.286
	(1.63)	(0.61)	(1.64)	(0.84)	(1.68)
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Coefficients obtained from a gamma distributed accelerated failure time model. Absolute values of asymptotic T statistics in parentheses. Joblessness set to 0.5 weeks for individuals out-of-work less than one week.

Table 4.4: Effect of Various Types of Advance Notice on Postdisplacement Joblessness (all spells)

Adv. Notice Regressor	All Workers	Males	Females	Plant Closings	Layoffs
	Subsample				
model one					
NOTICE	-0.085	-0.162	0.055	-8.4E-4	-0.132
	(1.02)	(1.56)	(0.39)	(0.01)	(1.08)
model two					
WRITTEN	0.186	0.086	0.313	0.129	0.286
	(1.63)	(0.61)	(1.64)	(0.84)	(1.68)
model three					
ADV0	-0.164	-0.226	-0.036	-0.047	-0.259
	(1.81)	(2.03)	(0.24)	(0.38)	(1.95)
ADV1	0.323	0.137	0.559	0.198	0.442
	(1.82)	(0.63)	(1.85)	(0.76)	(1.85)
ADV2	0.239	0.304	0.145	0.139	0.368
	(1.16)	(1.20)	(0.43)	(0.52)	(1.14)
ADV3	0.280	0.299	0.192	0.174	0.549
	(1.38)	(1.19)	(0.58)	(0.70)	(1.47)
Dep. Mean (weeks)	27.69	23.06	34.33	27.51	27.92
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Coefficients obtained from a gamma distributed accelerated failure time model. Joblessness set to 0.5 weeks for individuals out-of-work less than one week. Absolute values of asymptotic T statistics in parentheses.

2. Description of Advance Notice Dummy Variables. (Variable is equal to one if:)

NOTICE - "Expected" or received advance notice of job termination
WRITTEN - Received written advance notice of job termination
ADV0 - No written notice but received unwritten notice or expected job termination
ADV1 - Received less than one month's written advance notice
ADV2 - Received between one and two months written advance notice
ADV3 - Received more than two months written advance notice

Table 4.5:
Effect of Various Types of Advance Notice on Postdisplacement Joblessness
(spells ≥1 week)

Adv. Notice Regressor	Subsample All Workers	Males	Females	Plant Closings	Layoffs
model one					
NOTICE	0.111	0.060	0.171	0.172	0.040
	(1.48)	(0.66)	(1.37)	(1.63)	(0.37)
model two					
WRITTEN	0.320	0.171	0.489	0.275	0.339
	(3.11)	(1.35)	(2.81)	(1.96)	(2.22)
model three					
ADV0	0.023	0.007	0.052	0.104	-0.073
	(0.28)	(0.07)	(0.39)	(0.93)	(0.61)
ADV1	0.220	-0.006	0.483	0.140	0.265
	(1.41)	(0.03)	(1.83)	(0.62)	(1.24)
ADV2	0.216	0.289	0.091	0.096	0.341
	(1.17)	(1.27)	(0.30)	(0.40)	(1.16)
ADV3	0.611	0.430	0.863	0.550	0.753
	(3.19)	(1.86)	(2.68)	(2.36)	(2.17)
Dep. Mean (weeks)	31.10	26.53	37.29	31.31	30.83
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Coefficients obtained from a gamma distributed accelerated failure time model. Absolute values of asymptotic T statistics in parentheses.

2. Description of Advance Notice Dummy Variables. (Variable is equal to one if:)

NOTICE - "Expected" or received advance notice of job termination
WRITTEN - Received written advance notice of job termination
ADV0 - No written notice but received unwritten notice or expected job termination
ADV1 - Received less than one month's written advance notice
ADV2 - Received between one and two months written advance notice
ADV3 - Received more than two months written advance notice

Table 4.6:
Probit Regressions Indicating the Probability
of Working for Pay in January 1988

			Subsample		
Adv. Notice Regressor	All Workers	Males	Females	Plant Closings	Layoffs
model one					
NOTICE	0.023	-0.040	0.122	0.072	0.117
	(0.31)	(0.40)	(0.99)	(0.71)	(1.03)
model two					
WRITTEN	0.040	0.095	-0.023	0.064	0.021
	(0.39)	(0.69)	(0.13)	(0.47)	(0.13)
model three					
ADV0	0.021	-0.067	0.152	-0.095	0.140
	(0.26)	(0.63)	(1.15)	(0.87)	(1.11)
ADV1	0.075	0.117	0.056	0.123	0.028
	(0.47)	(0.54)	(0.21)	(0.54)	(0.12)
ADV2	-0.109	-0.208	0.011	-0.146	-0.053
	(0.63)	(0.93)	(0.04)	(0.64)	(0.19)
ADV3	0.140	0.230	0.053	-0.007	0.411
	(0.71)	(0.88)	(0.16)	(0.03)	(0.96)
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Regressions estimated by maximum likelihood probit. Absolute values of asymptotic T statistics in parentheses. Sample restricted to workers displaced between 1983 and 1985.

2. Description of Advance Notice Dummy Variables. (Variable is equal to one if:)

NOTICE - "Expected" or received advance notice of job termination
WRITTEN - Received written advance notice of job termination
ADV0 - No written notice but received unwritten notice or expected job termination
ADV1 - Received less than one month's written advance notice
ADV2 - Received between one and two months written advance notice
ADV3 - Received more than two months written advance notice

Chapter 5:

Advance Notice and Postdisplacement Wages

Research on the adjustment problems of displaced workers has mainly focused upon the initial period of joblessness experienced by most individuals. Although this concern is well placed, less attention has been paid to the wages received in subsequent positions. This latter issue is especially important given recent evidence indicating that although postdisplacement joblessness is generally transitory, the associated wage changes are more permanent — over 50 percent of the initial wage change persists for at least 5 years and 40 percent for decade or more (Ruhm 1989a). This chapter attempts to reduce our ignorance about wage effects by studying the relationship between advance notice and reemployment earnings. As in chapter 4, notification is assumed to be provided exogenously. The possibility of endogenous notification is reserved for chapter 6.

Potential wages of displaced workers, in January 1988, are assumed to follow the standard log-linear relationship:

$$W^* = Z_1\beta + \mu_1 . \tag{5.1}$$

where the dependent variable is the natural log of weekly potential wages, Z_1 is a vector of covariates, and μ_1 is an error term. One difficulty with estimating the effect of advance notice (or of other covariates) on wages is that potential wages are observed only for individuals who are reemployed at the time of the survey. Since approximately a quarter of respondents do not work in January 1988, potentially serious selection biases may result from estimating the standard linear regression model using either the subsample of reemployed workers or the full sample (with the wages of the nonemployed set to zero).

Techniques to correct for selection bias, pioneered by Heckman (1979), have become fairly standard. Individuals are assumed to be employed at the survey date if

$$I = Z_2\gamma + \mu_2 > 0 . \tag{5.2}$$

In (5.2), I is a latent variable, Z_2 the regression covariates, and μ_2 a disturbance term. Observed wages are described by

$$W = \begin{matrix} W^* \\ 0 \end{matrix} \quad \text{if} \quad I \begin{matrix} > \\ \leq \end{matrix} 0. \tag{5.3}$$

μ_1 and μ_2 are assumed to be bivariate normally distributed with $E(\mu_i) = 0$ and $\mathrm{cov}(\mu_i\mu_j) = \sigma_{ij}$. σ_{22} is not observed and is conventionally normalized to equal 1.

The potential selection bias is easily seen. In the ordinary least squares regression

$$W = Z_1\beta + \mu , \tag{5.4}$$

estimated on the subsample of reemployed workers,

$$E(\mu) = E(\mu_1|I>0) = \sigma_{12}[\phi(.)/\Phi(.)] ,$$

for $\phi(.)$ and $\Phi(.)$ the p.d.f. and c.d.f. of the standard normal distribution evaluated at $Z_2\gamma$. The error term has a zero mean only in the case where reemployment occurs randomly ($\sigma_{12} = 0$). Otherwise, the intercept term of the ordinary least squares regression will be biased and the regressor coefficients are likely to be estimated inconsistently as well.

Heckman suggested correcting for selection bias using a two-stage method. In the first stage, the equation

$$I^* = Z_2\gamma + \mu_2 > 0 \tag{5.5}$$

is estimated on the full sample using maximum likelihood probit techniques, where I* is a dummy variable equal to 1 (0) if observed wages are greater than (equal to) 0. Estimates for $\hat{\phi}(.)$ and $\hat{\Phi}(.)$ are then obtained from the probit coefficients. In the second stage, the wage regression

$$W = Z_1\beta + [\hat{\phi}(.)/\hat{\Phi}(.)]\lambda + \epsilon \tag{5.6}$$

is estimated using ordinary least squares for the subsample of reemployed workers.

The Heckman two-stage procedure provides consistent but inefficient parameter estimates. Greater efficiency can be obtained by estimating the combined system of equations (5.5) and (5.6) using maximum likelihood techniques. For this reason ML methods are utilized in this chapter.

5.1 Non-notification Regressors

Table 5.1 displays maximum likelihood estimates showing the effect individual, firm, and geographic covariates on the log of survey date wages, where written advance notice is the prenotification variable. Rho indicates the estimated correlation between ϵ_1 and ϵ_2. The selection and wage equations contain the same vector of regressors.[1]

Coefficients on the non-notification regressors are fairly standard. Postdisplacement wages are higher for married workers, household heads, and respondents with few children, than for their counterparts. This presumably indicates unobserved differences in labor force attachment and worker quality. Negative coefficients for nonwhites and females probably indicate some combination of unobserved heterogeneity and discrimination. Survey date earnings increase with labor market experience and education, presumably because each of these is correlated with investments in general human capital. Postseparation wages also rise with previous tenure, which confirms the findings of previous studies (Kletzer, 1989, Ruhm 1990) indicating that a portion of cross-sectional seniority differentials result because individuals possessing above average amounts of market-valued traits changes jobs relatively infrequently.

Factors associated with increased unionization are correlated with higher postdisplacement earnings. This may indicate that union wage premiums partly reflect productive worker differences. Similarly, between 35 and 50 percent of predisplacement wage residuals are retained in new positions, implying a substantial role for unobserved heterogeneity. High area or occupation unemployment rates and slow industry employment growth all lower postdisplacement earnings and indicate the importance of local and sectoral labor market conditions. Conversely, survey date wages appear higher for workers displaced by plant closings or eligible for unemployment insurance than for their counterparts, although these results are measured imprecisely.

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[1] As a result, model identification depends on the assumptions about the functional form and error distributions. We would have preferred to have excluded some regressors from either the selection or wage equations, however, this was not done because there were no variables which obviously influenced reemployment probabilities independent of reemployment wages or vice versa.

The effects of the covariates are remarkably similar across the gender and reason for displacement subsamples and it appears safe to assume that the same process determines postdisplacement earnings in all cases. These intergroup similarities are consistent with (but stronger than) those found in the previous chapter's analysis of joblessness.

The coefficients on RHO, show that the error terms in the selection and wage equations are only weakly and inconsistently related. The full sample correlation is only 0.065, with a standard error which is over 3 times as large. There is some evidence of positive selection for males, which implies that high earners are more likely to be reemployed in January 1988 than are their peers with lower potential wages. The coefficients also suggest positive selection for laidoff individuals and negative selection for females; however, these effects are measured imprecisely.

5.2 Advance Notice and Wages

Table 5.2 shows the relationship between various types of advance notice and postdisplacement wages, obtained by estimating the joint selection and wage equation using maximum likelihood. Each of the 3 models includes the common vector of control variables described in Table 5.1.

There is no evidence that the survey date wages of workers who expected job terminations differs from those fo whom it was a surprise. Using NOTICE as the notification criteria, the regressions indicate that prior notice is associated with an expected wage increase of less than 1 percent, with the standard error almost 4 times as large as the regression coefficient (see Table 5.2, row 1). The coefficients range between -3.1 and 2.4 percent for the 4 subsamples and never approach statistical significance. These results are consistent with the findings of Ehrenberg & Jakubson (1988) using the 1984 DWS. They find that notified workers earn between 3.2 percent less and 4.6 more than their counterparts. None of these differences are statistically significant.[2]

Row 2 of Table 5.2 shows that there is also no statistically discernible effect of written advance notice for most groups. The full sample coefficient is only 0.0013 and the predicted effects of written notice for the gender stratified subsamples range from -2.3 to 3.3 percent. The impact of formal notice does appear to depend upon the reason for displacement however. Persons displaced by plant closings obtain moderately higher wages (7.6%) if they receive written notice than if they do not, while workers involved in partial layoffs earn 10.0 percent less if they are formally notified. The layoff coefficient barely misses statistical significance.

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[2] The Ehrenberg & Jakubson estimates are obtained without correcting for selection bias. Their selection corrected differences range between -7.7 and 4.6 percent.

The results of model 3 indicate that although written notice has no average beneficial effect, formal announcements provided well in advance of the job terminations result in favorable wage outcomes. The full sample coefficient on ADV3 implies that workers with more than 2 months specific warning earn 13.3 percent more than peers who did not expect the job displacement, 12.6 percent greater than the informally notified, and 20.1 percent above those receiving written notification of less than 1 month (see rows 3 through 6). The anticipated gains are even larger for females and workers involved in plant closings and are close to being statistically significant for these subgroups and the full sample. On the other hand, the benefits of extensive formal notice are much smaller for males and laidoff respondents.

The small overall effect of written advance notice occurs because short periods of prenotification are generally associated with reductions in survey date earnings. Workers receiving written warning of less than 1 month are predicted to earn 5.6 percent less than their non-notified counterparts. Among the subsamples, the effects of short written notice range from a statistically insignificant gain of 3.9 percent for workers displaced by plant closings to a large and statistically significant loss of 13.9 percent for laidoff individuals. Since it seems implausible that short periods of notice cause relative wage declines, the negative coefficients are likely to indicate the endogeneity of advance notice. This issue is addressed in chapter 6.

5.3 Summary

Workers expecting or obtaining advance notice of displacements earn virtually identical survey date wages to their counterparts for whom the job terminations came as a surprise. Whereas there is also no evidence that written notification provides larger benefits than informal notice, on average, respondents obtaining lengthy periods of written warning do earn more in postdisplacement jobs than other workers. Especially large benefits are observed for females and persons displaced during plant shutdowns. A corollary of this result is that individuals formally notified shortly before the job loss earn relatively low survey date wages.

These results indicate that although workers expecting job terminations find new positions faster than their counterparts, the speedier reemployment does not translate into higher postdisplacement wages. Conversely, lengthy written notice appears to improve subsequent earnings, despite having little effect on the duration of joblessness.[3] The negative coefficients on formal notice of less than 1 month suggest potential endogeneity biases and point out the importance of more sophisticated procedures which attempt to account for them.

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[3] As shown in Table 4.2, however, the duration of written notice may be positively related to the possibility of avoiding nonemployment.

Table 5.1:
Postdisplacement Wage Equations with Correction for Selection Bias

	Subsample				
Regressor	All Workers	Males	Females	Plant Closings	Layoffs
EXP	0.021	0.020	0.025	0.026	0.015
	(3.12)	(2.23)	(2.13)	(2.96)	(1.32)
EXPSQ	-5.7E-4	-5.6E-4	-6.2E-4	-6.8E-4	-4.4E-4
	(3.77)	(2.88)	(2.31)	(3.49)	(1.71)
EDUC	-0.045	-0.033	-0.084	-0.037	-0.035
	(1.05)	(0.65)	(0.91)	(0.71)	(0.42)
EDUCSQ	0.004	0.004	0.005	0.004	0.004
	(2.57)	(2.01)	(1.54)	(1.75)	(1.39)
TENURE	0.021	0.022	0.015	0.021	0.020
	(2.86)	(2.57)	(0.88)	(2.11)	(1.64)
TENSQ	-7.6E-4	-8.0E-4	-7.1E-4	-5.9E-4	-1.2E-3
	(3.28)	(3.00)	(0.96)	(1.82)	(2.64)
MARRIED	0.117	0.145	0.107	0.135	0.087
	(2.70)	(2.16)	(1.10)	(2.49)	(1.18)
HEAD	0.218	0.127	0.189	0.283	0.149
	(4.76)	(1.56)	(1.94)	(4.45)	(2.19)
FEMALE	-0.310			-0.274	-0.355
	(8.58)			(5.69)	(6.26)
BLACK	-0.089	-0.096	-0.060	-0.154	-0.023
	(1.59)	(1.45)	(0.55)	(1.87)	(0.28)
OTHRACE	-0.092	-0.070	-0.143	-0.118	-0.065
	(1.24)	(0.76)	(0.99)	(1.22)	(0.45)
NUMCHILD	-0.052	-0.040	-0.075	-0.049	-0.057
	(3.81)	(2.51)	(2.80)	(2.69)	(2.74)
NUMEARN	-9.7E-3	0.019	-0.050	-0.017	0.017
	(0.25)	(0.45)	(0.87)	(0.34)	(0.30)
PRTPREV	0.032	0.173	0.016	0.199	-0.152
	(0.52)	(1.70)	(0.16)	(2.46)	(1.53)
HINS	0.046	0.060	0.024	0.022	0.071
	(1.27)	(1.24)	(0.39)	(0.45)	(1.24)
CLOSING	0.052	0.041	0.056		
	(1.85)	(1.14)	(1.14)		
RESID	0.418	0.392	0.427	0.486	0.355
	(13.27)	(10.88)	(7.27)	(10.43)	(7.96)
STEUNION	0.010	0.011	9.1E-3	4.2E-3	0.016
	(3.72)	(3.06)	(2.04)	(1.08)	(4.08)
RTWORK	-0.031	-2.8E-3	-0.069	-0.113	0.052
	(0.78)	(0.05)	(1.03)	(2.03)	(0.86)

URATE	0.049	0.055	0.044	0.042	0.053
	(3.26)	(2.86)	(1.58)	(2.10)	(2.20)
AREARATE	-0.031	-0.033	-0.033	-0.029	-0.030
	(4.23)	(3.52)	(2.60)	(3.10)	(2.47)
INDCHG	0.012	7.8E-3	0.023	0.029	-5.1E-3
	(1.64)	(0.85)	(1.54)	(2.76)	(0.42)
OCCRATE	-0.020	-0.014	-0.036	-0.016	-0.025
	(3.51)	(1.99)	(2.95)	(2.00)	(2.83)
INDUNION	2.2E-3	2.1E-3	2.4E-3	3.2E-3	1.6E-3
	(1.45)	(1.07)	(0.89)	(1.60)	(0.61)
OCCUNION	2.5E-3	2.2E-3	4.0E-3	3.7E-3	1.1E-3
	(1.73)	(1.27)	(1.28)	(1.75)	(0.52)
UIPROB	0.178	0.173	0.247	0.180	0.180
	(1.44)	(1.17)	(1.13)	(1.12)	(0.88)
WRITTEN	1.3E-3	0.032	-0.023	0.073	-0.105
	(0.04)	(0.65)	(0.32)	(1.27)	(1.90)
RHO	0.065	0.379	-0.199	-0.037	0.306
	(0.29)	(1.85)	(0.66)	(0.13)	(0.96)
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Coefficients show effects of parameters on the natural log of real postdisplacement weekly wages. They are obtained using maximum likelihood techniques on a bivariate normally distributed system of a selection equation and wage equation. The selection equation contains the same regressors as shown above. RHO is the estimated correlation between the error terms of the two equations. Absolute values of asymptotic T statistics in parentheses. The sample is restricted to persons displaced between 1983 and 1985.

Table 5.2:
The Impact of Various Types of Advance Notice on Postdisplacement Earnings

Adv. Notice Regressor	Subsample				
	All Workers	Males	Females	Plant Closings	Layoffs
model one					
NOTICE	0.006	0.024	-0.032	0.016	-0.014
	(0.22)	(0.70)	(0.64)	(0.43)	(0.33)
model two					
WRITTEN	1.3E-3	0.032	-0.023	0.073	-0.105
	(0.04)	(0.65)	(0.32)	(1.27)	(1.90)
model three					
ADV0	6.2E-3	0.020	-0.033	-6.6E-3	0.018
	(0.21)	(0.55)	(0.62)	(0.17)	(0.38)
ADV1	-0.058	-0.011	-0.113	0.038	-0.150
	(1.03)	(0.16)	(1.03)	(0.34)	(2.24)
ADV2	-0.035	0.066	-0.176	-4.3E-3	-0.096
	(0.49)	(0.73)	(1.27)	(0.04)	(0.87)
ADV3	0.125	0.070	0.212	0.159	0.039
	(1.69)	(0.79)	(1.45)	(1.78)	(0.23)
	(1)	(2)	(3)	(4)	(5)

Notes:

1. Coefficients show effects of advance notice on the natural log of postdisplacement weekly real wages. They are obtained using maximum likelihood techniques on a bivariate normally distributed system of a selection equation and wage equation. The selection equation contains the same regressors as shown above. Absolute values of asymptotic T statistics in parentheses. The sample is restricted to persons displaced between 1983 and 1985.

2. Description of Advance Notice Dummy Variables. (Variable is equal to one if:)

   NOTICE - "Expected" or received advance notice of job termination
   WRITTEN - Received written advance notice of job termination
   ADV0 - No written notice but received unwritten notice or expected job termination
   ADV1 - Received less than one month's written advance notice
   ADV2 - Received between one and two months written advance notice
   ADV3 - Received more than two months written advance notice

Chapter 6:

Endogeneity of Notice and Interaction With Covariates

Previous chapters have examined the effect of prior notification on postdisplacement employment conditions under the assumptions that early notice is provided exogenously and impacts only the intercept terms of the nonemployment and wage equations. Both of these assumptions are relaxed in this chapter. Evidence presented below suggests that the effects of prenotification on subsequent joblessness vary substantially across population subgroups while endogeneity biases are fairly minor. Conversely, endogeneity is more important when considering survey date earnings but intergroup variations in the impacts of prenotification are less substantial. We begin by presenting the estimation methodology and results for postdisplacement joblessness. The discussion of wage changes follows subsequently.

6.1 Switching Regression Model of Postdisplacement Joblessness

Postdisplacement joblessness for notified workers is specified by

$$W_1 = X \beta_1 + \mu_1 , \tag{6.1}$$

while that of non-notified individuals is

$$W_0 = X \beta_0 + \mu_0 . \tag{6.2}$$

Individuals are assumed to receive advance notice if the resulting reduction in joblessness ($W_0 - W_1$), outweigh any costs (e.g. lower wages, poorer working conditions) incurred to obtain it. These costs depend on personal characteristics and geographic factors and are characterized by

$$C = V \beta_2 + \rho \,, \tag{6.3}$$

where V includes all the elements in X plus additional regressors indicating residence in a state with mandatory or voluntary advance notice provisions.

Respondents are therefore prenotified notice if:

$$\begin{aligned} W_0 - W_1 - C &\geq 0 \,, \\ Z \gamma &\geq \mu_2 \,, \end{aligned} \tag{6.4}$$

where $Z \gamma = X (\beta_0 - \beta_1) - V \beta_2$ and $\mu_2 = \mu_1 - \mu_0 + \rho$. The expected joblessness of notified workers is:

$$E(W_1 | N=1) = X \beta_1 + E(\mu_1 | N=1) \;; \tag{6.5}$$

that of non-notified workers is:

$$E(W_0 | N=0) = X \beta_0 + E(\mu_0 | N=0) \,. \tag{6.6}$$

Reduced form coefficients obtained from regressing W on Z will therefore be biased except when switching is exogenous (which requires that $E(\mu_1 | N=1) = E(\mu_0 | N=0) = 0$). If μ_0, μ_1, and μ_2 are trivariate normally distributed with $E(\mu_i) = 0$ and $\text{cov}(\mu_i \mu_j) = \sigma_{ij}$, (6.7a) $$E(\mu_1 | N=1) = \sigma_{12}[-\phi(.)/\Phi(.)]$$

(6.7b $$E(\mu_0 | N=0) = \sigma_{02}[\phi(.)/(1-\Phi(.))],$$

for $\phi(.)$ and $\Phi(.)$ the p.d.f. and c.d.f. of the standard normal distribution evaluated at $Z\ \gamma$. Equations 6.7a) and 6.7b) imply that switching is endogenous when σ_{12} or σ_{02} is non-zero.

Consistent regression estimates can be obtained using the two-stage technique discussed by Lee (1978).[1] In the first stage, the structural probit equation implied by 6.4) is estimated to obtain $\hat{\phi}(.)$ and $\hat{\Phi}(.)$. Next the equations

(6.8a) $$W_1 = X\ \beta_1 + \lambda_1\ \gamma_1 + u_1$$

(6.8b) $$W_0 = X\ \beta_0 + \lambda_0\ \gamma_0 + u_0\ ,$$

are estimated by ordinary least squares, where $\lambda_1 = -\hat{\phi}(.)/\hat{\Phi}(.)$, $\lambda_0 = \hat{\phi}(.)/[1-\hat{\Phi}(.)]$, $\gamma_1 = \hat{\sigma}_{12}$, and $\gamma_0 = \hat{\sigma}_{02}$.[2]

We are also interested in examining the interactions between advance notice and the other covariates. Although these could be inferred from $\hat{\beta}_1 - \hat{\beta}_0$, a different procedure is used which allows them to be estimated directly. Combining equations (6.1) and (6.2), observed joblessness can be rewritten as:

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[1] Also see Willis & Rosen (1979) or Robinson & Tomes (1982) for early examples.

[2] The standard errors will be understated if account is not taken for heteroscedasticity of the error terms. Preliminary work indicated that the difference between OLS and GLS estimates was fairly small and OLS estimates are presented below.

$$W = X\ \beta_0 + X{*}N(\beta_1 - \beta_0) + \mu_0(1-N) + \mu_1\ N\ . \tag{6.9}$$

Expected duration, conditional on notification status, is then

$$\begin{aligned} E(W|N) &= X\ \beta_0 + X{*}N(\beta_1 - \beta_0) + E[\mu_0|N=0](1-N) + E[\mu_1|N=1]N\ , \\ &= X\ \beta_0 + X{*}N(\beta_1 - \beta_0) + \sigma_{02}[\phi(.)/(1-\Phi(.))](1-N) + \sigma_{12}[-\phi(.)/\Phi(.)]N \end{aligned} \tag{6.10}$$

and consistent estimates can be obtained from OLS regressions of

$$W = X\ \alpha_0 + X{*}N\ \alpha_1 + \lambda_0\ \alpha_2 + \lambda_1\ \alpha_3 + \mu\ , \tag{6.11}$$

where $\hat{\alpha}_0 = \hat{\beta}_0$, $\hat{\alpha}_1 = \hat{\beta}_1 - \hat{\beta}_0$, $\hat{\alpha}_2 = \hat{\sigma}_{02}$, $\hat{\alpha}_3 = \hat{\sigma}_{12}$, $\lambda_1 = [-\phi(.)/\Phi(.)]N$, and $\lambda_0 = [\phi(.)/(1-\Phi(.))](1-N)$.

6.2 Alternative Models

The switching regression model developed in section 6.1 assumes that the duration of joblessness is linear in the covariates. This differs from typical duration models, such as the accelerated failure time model used in Chapter 4, both in its functional form and because it fails to take special account of the right-censoring of nonemployment spells which occurs because some spells conclude after the date of the survey. To investigate whether linearity imposes unreasonable restrictions, reduced form estimates of the determinants of the duration of joblessness for a variety of models are first presented. The coefficient estimates are consistent across functional forms and indicate that the linear model is acceptable for more sophisticated analysis. If anything, OLS appears more appropriate than models which assume that the natural log of nonemployment duration is linear in the covariates (e.g. the accelerated failure time model).

5 models are compared below. These include the: linear, log-linear, two-limit tobit, accelerated failure time, and proportional hazards models. The generalized gamma accelerated failure time framework has been discussed in chapter 4. The linear and log-linear models are completely standard, taking the form:

$$W = Z \beta + \mu \tag{6.12}$$

where W is the level (natural log) of joblessness in the former (latter) case and μ is a white noise disturbance.

In the two-limit tobit model, a latent variable for joblessness is defined by:

$$I^* = Z \beta + \mu,$$

with the observed nonemployment (W) described by:

$$W = \begin{cases} 0 & \mu < -Z\beta \\ I^* \quad \text{if} & 99 - Z\beta > \mu > -Z\beta, \\ 99 & \mu > 99 - Z\beta \end{cases}$$

with μ normally distributed, and 0 (99) weeks the lower (upper) limit.

The proportional hazard model is specified by:

$$\ln h(w) = h_0(w)\, Z\beta \,, \tag{6.13}$$

where the hazard rate $h(w)$ is the probability of leaving joblessness at week w, conditional on remaining out-of-work through the end of $w-1$ [$h(w) = \Pr(W = w | W > w-1$] and h_0 is the baseline hazard function. The proportional hazard assumption implies that the covariates change the log hazard rate by a fixed proportion across time. Hazard models are frequently estimated by imposing parametric assumptions on the base line hazard. This paper uses the more general semi-parametric partial likelihood estimator (first proposed by Cox 1972), which only requires information on the rank-ordering of survival times.[3]

Regression coefficients are expected to be similar in the linear and two-limit tobit models, since the level of joblessness is the dependent variable in both cases, and also in the log-linear and accelerated failure time

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[3] The log-likelihood function (without ties) is:

$$L = \sum_{i=1}^{n} [Z_i\beta - \ln \Sigma_{j \in R_i} \exp(Z_j\beta)] \,,$$

where R_i is the set of individuals at risk at week w_i (those with $W \geq w_i$).

frameworks, where the dependent variable is the log of durations. The coefficients should have the opposite signs in the proportional hazard model because factors which increase the hazard rate (the probability of leaving joblessness) reduce nonemployment durations.

Results from the 5 models, presented in Table 6.1, indicate that the predicted determinants of joblessness are extremely robust to changes in the estimation technique. Household status, sex, race, number of children and earners, area unemployment rate, and the reason for displacement are highly significant and have consistent signs in each model. Residence in right-to-work states barely misses statistically significance in only the tobit model, while the industry growth rate is measured with slightly greater precision in the accelerated failure time and log-linear regressions than in the others. The t statistic on written advance notice is slightly larger for the linear model than the alternatives but the direction and general size of the findings is similar.

The root mean square error (RMSE) is slightly smaller in the linear than the tobit model and much below those in the log-linear or accelerated failure time frameworks. Similarly, the mean predicted joblessness of 27.63 weeks is exactly equal to the sample average for the linear case, slightly understated in the tobit model (27.50 weeks), and significantly underestimated in the two regressions where the dependent variable is the log of durations (12.22 and 16.35 weeks respectively). Because OLS is designed to minimize the RMSE, its superior performance by this criteria is expected. Even so, the large difference between predicted and actual average joblessness in the accelerated failure time and log-linear models suggests serious misspecification when log durations are used as the dependent variable.

Duration models might still be preferred because of their greater facility for coping with censored data. To examine this possibility, we consider the frequency with which the various models correctly predict right censoring (durations above 99 weeks) and the number of instances where OLS incorrectly forecasts negative joblessness (see the last 3 rows of Table 6.1). None of the estimation techniques is able to anticipate extended joblessness with any accuracy. Predicted joblessness is always less than 99 weeks in the linear, log-linear, and tobit models. The accelerated failure time formulation correctly forecasts right-censoring in 6 of 246 cases (2.4 percent) but incorrectly predicts it almost as often (for 4 individuals). Conversely, OLS forecasts negative durations for only 25 respondents (1.3 percent of the sample).

These results suggest that the advantages of the linear model, in allowing for endogenous regressors, outweigh the potential costs of model misspecification. They also indicate that greater attention needs to be paid to the somewhat restrictive functional forms used in conventional duration models.[4]

6.3 Switching Regression Results — Postdisplacement Joblessness

Tables 6.2 and 6.3 display regression coefficients on X*N, λ_1, and λ_2 from switching regression estimates of equation (6.11), obtained using the aforementioned two-stage procedure. The switching criteria is WRITTEN in

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[4] Addison & Portugal (1987b) examine alternative distributional assumptions within the accelerated failure time framework. They do not, however, consider alternative models.

Table 6.2 and NOTICE in Table 6.3. The selection coefficients provide no evidence that workers choose positions in order to minimize postdisplacement joblessness. This would require $\sigma_{12} > 0$ for notified workers and $\sigma_{02} < 0$ for non-notified individuals — which would imply positive (negative) regression coefficients on λ_1 (λ_0). Instead, the imprecisely measured Mill's coefficients provide weak evidence that workers, both with and without written advance notice experience more extended joblessness, than if their notification status were reversed (see Table 6.2). Table 6.3 reveals an even less consistent pattern for the broader definition of prior notice. Again there is no evidence that postlayoff joblessness is a key determinant of the type of predisplacement employment chosen.

More generally, the low statistical significance of the selection coefficients implies that the switching model only partially solves the endogeneity problem. At least two possibilities deserve further consideration. First, factors other than postdisplacement joblessness (e.g. wages and fringe benefits) may play a more crucial role in determining whether individuals accept positions offering prior notification. This would be consistent with the evidence that notified workers have relatively high rates of health insurance coverage and receive positive wage residuals in predisplacement jobs. Alternatively, positions providing notice may be rationed such that firm rather than individual characteristics are of key importance. For example, large or unionized firms may frequently provide advanced warning and, if they also pay efficiency wages, have job queues. Because the DWS contains relatively sparse firm level information, the analysis will then fail to capture important sources of endogeneity.

The switching regression interaction terms reveal significant differences in the effects of prior notification across population subgroups. Particularly interesting are the large reductions in joblessness associated with written advance notice among married persons, household heads, and displaced workers living in areas with high unemployment (see Table 6.2). For instance, formal notice decreases the expected nonemployment of married household heads residing in areas with local unemployment rates 5 percent above the national average by over 10 weeks, compared to their non-notified counterparts; the corresponding reduction for the same individuals living in areas with unemployment 7.5 percent greater than average is almost 20 weeks.[5] Relatively large reductions are also observed for highly educated workers receiving written notice.

Interestingly, none of the above groups exhibit markedly reduced joblessness when the broader measure of early notice, which includes informal notification, is employed (see Table 6.3). This further qualifies the usefulness of previous research which has relied on the more general benchmark. Table 6.2 also provides some indication that written warning leads to relatively large beneficial effects for nonwhites, females, and workers displaced because of plant closings or from fast growing industries. These effects are measured imprecisely, however, and so should be considered preliminary.

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[5] All other regressors are evaluated at the sample means. Howland & Peterson (1988) have previously documented the importance of local labor market conditions in determining the reemployment success of displaced workers.

6.4 Postdisplacement Earnings

We next use the switching framework to consider the impact of endogenously provided advance notice on survey date earnings. The basic model is the same as above, except that we now also allow for selection bias in the reemployment equation (as discussed in chapter 5). Using the standard two-stage "Heckman" correction for selection bias, the wage equations are:

$$W_1 = X\,\beta_1 + \lambda_w\,\gamma_1 + \mu_1\ . \tag{6.14}$$

for notified workers and

$$W_0 = X\,\beta_0 + \lambda_w\,\gamma_0 + \mu_2, \tag{6.15}$$

for non-notified individuals. λ_w is the inverse Mills ratio from the reemployment wage equation and W now indicates real weekly wages in January 1988. We again directly obtain estimates of interaction effects between N and X (and λ_w) by estimating the single equation:

$$W = X\,\beta_0 + X{*}N\,(\beta_1-\beta_0) + \lambda_w\,\gamma_0 + X{*}N\,(\beta_1-\beta_0) + \lambda_w{*}N\,(\gamma_1-\gamma_0) + \upsilon\ . \tag{6.16}$$

The inclusion of λ_w as a separate regressor is appropriate only if the error terms from the reemployment and switching regressions are independent.[6] To provide an indication whether is the case, we first estimated, by maximum likelihood, a bivariate probit model consisting of the reemployment and switching equations, with NOTICE and WRITTEN as the notification variables. The estimated correlations between the disturbance terms, which are shown in Table 6.4, are always extremely small and the t statistic never exceeds 0.3. This suggests that the assumption of independence is reasonable.[7]

Table 6.5 presents estimates of the interaction terms of switching model described by equation (6.16). Three types of advance notice — NOTICE, WRITTEN, and ADV3 — are included as switching criteria. Since the reduced form estimates, in chapter 5, indicated that the regression covariates had fairly similar effects across the gender and reason for displacement subsamples, the table presents results for only the full sample.[8] Table 6.6 show estimates for a model which is identical to that in table 6.5, except that the inverse Mills variables are excluded. The findings are virtually identical in the two cases, which indicates that reemployment selection biases are relatively minor and that the results of the "Heckman" corrected regressions do not depend on specific assumptions about the error distributions.

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[6] If they are not, the system of reemployment and switching equations should be estimated as a bivariate probit model using maximum likelihood techniques. Wymand & Bernard (1981) propose an estimator which is appropriate in this case.

[7] An alternative test involves examining whether the value of $\hat{\gamma}_1 - \hat{\gamma}_0$ is close to zero in estimates of (6.16). As shown below, this test also supports the independence assumption.

[8] The switching model was separately estimated for males, females, laidoff workers, and those involved in plant closings, with similar coefficients on the covariates to the full sample results displayed in the table.

Section 6.3 indicated that written advance notice strongly reduces the postdisplacement joblessness of some groups (household heads, married persons, and residents of economically depressed localities) while having little or no effect on others. There is less evidence of a differential impact of prenotification on survey date earnings. None of the interaction coefficients are statistically significant when WRITTEN is used as the notification criteria and only those for job tenure when ADV3 is employed. Intergroup differences appear more important when NOTICE is utilized as the switching variable; however, the vagueness of this criteria again makes it difficult to interpret the meaning of the disparities.

Table 6.7 displays estimates for the switching coefficients (λ_0 and λ_1) and inverse Mills ratio (λ_w) for the full sample as well as for subsamples stratified by sex and reason for displacement. The coefficient on λ_w differs insignificantly from 0 in all the regressions, which is consistent with the earlier evidence indicating that reemployment occurs independently of potential wages.

Recalling that the coefficients on λ_0 and λ_1 show estimated values of σ_{02} and σ_{12}, respectively, the negative full sample values for both parameters imply that the expected value of the error term on the survey date wage equation is positive (negative) for notified (non-notified) workers. This implies that the average displaced worker would obtain a smaller wage benefit from advance warning than does the typical respondent who actually received early notice and suggests that the reduced form wage regressions overstate the beneficial effects of mandatory advance notice. The switching coefficients also indicate the importance of unobserved heterogeneity, where workers with greater endowments of market valued traits are more likely to receive advance notice than are their less desired counterparts. The coefficients on λ_0 and λ_1 vary across subsamples but are generally measured sufficiently imprecisely as to prevent interpretation of the differences.

To provide a summary measure of the effect of advance notice on survey date earnings, I reestimated the switching model with the coefficients on the non-notification covariates constrained to be equal in the two wage equations and the impact of prior notification restricted to an intercept effect. The second stage regression equation in this model is:

$$W = X\beta + \lambda_w \gamma + N\beta_n + \lambda_0\sigma_{02} + \lambda_1\sigma_{12} + \nu \,, \tag{6.17}$$

with β_n indicating the average impact of advance notice on the natural log of weekly wages. Table 6.8 displays estimated coefficients on β_n, λ_0, and λ_1 obtained from regressions of (6.17).

The switching parameters continue to indicate negative selection for non-notified workers and positive selection for notified respondents, when NOTICE or WRITTEN are the switching criteria, but the effects are imprecisely measured ADV3 is used (see Table 6.8, column 1). Given the direction of endogeneity bias, it is not surprising to find that the predicted notification effects are much smaller in these estimates than in the reduced form regressions of Chapter 5. What is less explicable are the large negative values on the coefficients. For example, the full sample estimate implies that written advance notice causes survey date earnings to be reduced by approximately two-thirds. In the absence of a mechanism by which prenotification could dramatically worsen outcomes, such a large negative impact is clearly implausible.[9] We therefore conclude that although the switching regression model provides some information on the nature of endogeneity bias, the the resulting estimates of the impact of prenotification are unreliable.

Three additional points deserve mention. First, written notice of more than 2 months continues to have a large (although imprecisely measured) beneficial effect on survey date wages (see row 7). This is consistent with evidence of the previous chapter indicating the more favorable impact of lengthy than short formal notification. Second, written notice continues to have a larger salutary impact on survey date earnings following plant closings than layoffs (columns 4 and 5). Third, the switching coefficients on the layoff subsample underscore the failure of this method to fully account for endogeneity bias. Recalling that the reduced form estimates of chapter 5 indicated a negative impact of written advance notice on this group, which was attributed to endogeneity bias, the switching model regressions were expected to result in more positive predicted outcomes. Instead, the regressions indicate negative selection for non-notified workers and positive selection for notified individuals, when using NOTICE or WRITTEN as the switching criteria, which leads to even larger reductions in predicted earnings for prenotified respondents. The findings are more promising, but hardly conclusive, when formal notice exceeding 2 months is considered.

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[9] The predicted effect is virtually identical when interactions between advance notice and the full vector of covariates (evaluated at the sample means) are allowed for. Ehrenberg & Jakubson's (1988) attempts to endogenize advance notice also result in substantial negative predicted impacts in several cases.

6.5 Summary

Prior to 1989, advance notification was mandatory in only 3 states. Reduced form estimates of the impact of voluntary prior notice on postdisplacement wages and joblessness may therefore suffer from endogeneity biases which limit their ability to predict the effects of mandated prenotification. This chapter presents a preliminary attempt to eliminate these biases, in the form of a switching regression model with endogenous switching.

Since the switching framework assumes that the dependent variable is linear in the covariates, we first tested whether the assumption of linearity was unreasonable when considering postdisplacement joblessness — where some observations are right censored durations or truncated at zero. The determinants of nonemployment were found to be robustly estimated across a wide variety of models. If anything, models which assume the dependent variable is linear in the covariates (e.g. OLS, tobit) performed better than those where the log of joblessness is linearly related to the regressors (log-linear, accelerated failure time).

Written advance notice leads to relatively large reductions in nonemployment durations for three groups of special concern — married persons, household heads, and displaced workers located in regions of high unemployment — as well as for highly educated individuals. Where intergroup differences in the effects of formal notice were less pronounced for survey date earnings than postdisplacement joblessness, endogeneity bias is of greater concern. Specifically, individuals receiving voluntarily provided advance notice benefit from it more than the average displaced worker would be expected to. This implies that reduced form estimates of chapter 5 overstate the beneficial wage impacts of prenotification. Unfortunately, the switching estimates imply that early notice (expect for written announcements over 2 months before displacements) is associated with implausibly large reductions in earnings. The predicted wage effects, from this model, must therefore be considered unreliable. Pending further study, it seems most reasonable to assume that estimates from reduced form wage equations in chapter 5 represent an upper bound on the benefits of written notification for survey date earnings. Further research to obtain a corresponding lower bound is clearly needed.

Table 6.1:
Estimates of Postdisplacement Joblessness
Using Various Regression Models

			Estimation Model		
Regressor	Linear	Tobit	Acc. Failure	Log-Linear	Prop-Hzrd
EXP	0.105	0.106	0.026	0.033	-0.006
	(0.32)	(0.26)	(1.35)	(1.96)	(0.51)
EXPSQ	0.009	0.012	6.6E-5	-2.5E-4	-2.6E-4
	(1.28)	(1.43)	(0.16)	(0.67)	(0.96)
EDUC	-0.863	-0.463	0.003	0.012	0.011
	(0.48)	(0.21)	(0.03)	(0.12)	(0.16)
EDUCSQ	0.013	-0.016	-0.001	-0.002	8.9E-5
	(0.19)	(0.19)	(0.01)	(0.45)	(0.03)
TENURE	0.517	0.401	0.018	0.009	-0.015
	(1.39)	(0.84)	(0.83)	(0.47)	(1.15)
TENSQ	0.006	0.015	5.1E-4	4.7E-4	-2.0E-4
	(0.48)	(0.93)	(0.68)	(0.70)	(0.43)
MARRIED	2.230	2.368	0.096	-0.015	-0.048
	(1.09)	(0.93)	(0.31)	(0.14)	(0.67)
HEAD	-12.343	-15.738	-0.731	-0.598	0.445
	(6.07)	(5.83)	(6.14)	(5.63)	(5.93)
FEMALE	6.340	9.051	0.396	0.326	-0.216
	(3.26)	(3.44)	(3.50)	(3.21)	(3.04)
BLACK	11.960	15.999	0.639	0.480	-0.405
	(4.84)	(5.48)	(4.38)	(3.71)	(4.35)
OTHRACE	17.689	22.293	1.002	0.880	-0.624
	(4.32)	(4.99)	(4.16)	(4.11)	(4.05)
NUMCHILD	3.247	4.225	0.161	0.119	-0.105
	(5.15)	(5.53)	(4.25)	(3.62)	(4.56)
NUMEARN	-7.075	-9.219	-0.363	-0.270	0.222
	(8.03)	(8.25)	(7.12)	(5.86)	(7.16)
PRTPREV	4.226	2.718	0.097	0.046	-0.140
	(1.30)	(0.75)	(0.51)	(0.27)	(1.18)
HINS	-2.743	-3.397	-0.109	-0.069	0.073
	(1.52)	(1.43)	(1.04)	(0.73)	(1.12)
CLOSING	-4.116	-5.413	-0.268	-0.260	0.122
	(2.78)	(2.84)	(3.12)	(3.36)	(2.35)
RESID	-0.577	-1.974	-0.122	-0.126	0.043
	(0.34)	(0.86)	(1.24)	(1.43)	(0.70)
STEUNION	-0.108	-0.126	-0.010	-0.011	0.003
	(0.80)	(0.73)	(1.28)	(1.53)	(0.63)
RTWORK	-4.278	-4.597	-0.262	-0.232	0.154
	(2.09)	(1.71)	(2.20)	(2.16)	(2.14)
URATE	-0.624	-0.994	-0.020	-0.015	-7.1E-4
	(0.82)	(1.02)	(0.45)	(0.37)	(0.03)
AREARATE	2.130	2.641	0.104	0.079	-0.064
	(5.87)	(5.96)	(4.95)	(4.19)	(4.92)

INDCHG	-0.575	-0.727	-0.051	-0.051	0.025
	(1.52)	(1.45)	(2.32)	(2.58)	(1.87)
OCCRATE	0.111	0.129	0.008	0.008	-0.004
	(0.34)	(0.29)	(0.42)	(0.48)	(0.35)
INDUNION	-0.034	-0.032	-0.002	-0.002	0.002
	(0.43)	(0.31)	(0.44)	(0.46)	(0.71)
OCCUNION	0.038	0.060	0.003	0.002	-0.002
	(0.51)	(0.60)	(0.70)	(0.63)	(0.77)
UIPROB	-3.279	-3.918	0.165	0.349	-0.053
	(0.57)	(0.49)	(0.49)	(1.15)	(0.25)
WRITTEN	4.380	4.554	0.186	0.135	-0.133
	(2.24)	(1.88)	(1.63)	(1.32)	(1.88)
RMSE	29.885	30.023	32.398	34.327	
Predicted Joblessness:					
mean duration:	27.63 wks	27.50 wks	16.35 wks	12.22 wks	
weeks > 99:					
correct	0	0	6	0	
incorrect	0	0	4	0	
weeks < 0:	25				

Notes:

1. Absolute values of asymptotic t statistics in parentheses. N = 1873; 13.1% of observations are censored at 99 weeks. Average joblessness is 27.63 weeks.

2. Description of estimation technique, dependent variable for various models:

Linear: estimation = ordinary least squares; dep. var. = survival time
Acc. Failure: estimation = acc. failure time; dep. var. = log survival time
Prop. Hzrd: estimation = Cox proportional hazard; dep. var. = hazard rate
Tobit: estimation = two-limit tobit; dep. var. = survival time
Log-Linear: estimation = ordinary least squares; dep. var. = log survival time

3. Spell durations set to 0.5 weeks in the acc. failure, prop. hzrd., and log-linear models for persons out-of-work less than one week.

Table 6.2:
Switching Regression Estimates for Postdisplacement Joblessness (Switching Criteria = Written Notice)

Regressor	All Workers	Males	Females	Closings	Layoffs
EXP	-0.707	-2.094	-0.094	-1.033	0.822
	(0.63)	(1.71)	(0.05)	(0.67)	(0.47)
EXPSQ	0.014	0.044	-0.022	0.021	-0.040
	(0.64)	(1.67)	(0.61)	(0.72)	(0.87)
EDUC	-12.894	-7.605	-22.468	-14.827	3.531
	(1.86)	(0.78)	(1.91)	(1.89)	(0.20)
EDUCSQ	0.501	0.248	0.992	0.558	-3.2E-3
	(2.01)	(0.72)	(2.10)	(1.91)	(0.01)
TENURE	-0.322	-0.605	1.489	0.503	-1.026
	(0.22)	(0.47)	(0.63)	(0.23)	(0.47)
TENSQ	0.032	0.026	0.087	-6.8E-4	0.104
	(0.97)	(0.69)	(1.10)	(0.01)	(1.26)
MARRIED	-15.172	-5.986	-49.015	-14.513	-18.644
	(2.40)	(0.73)	(2.44)	(1.97)	(1.88)
HEAD	-20.329	-22.581	-29.140	-21.348	-14.725
	(3.24)	(1.77)	(2.01)	(2.47)	(1.54)
FEMALE	-6.332			-6.995	-3.009
	(0.67)			(0.72)	(0.30)
BLACK	-7.798	-5.577	-36.189	-16.763	1.781
	(1.04)	(0.60)	(2.13)	(1.76)	(0.14)
OTHRACE	-5.149	-16.064	-0.418	-11.510	15.252
	(0.39)	(1.06)	(0.02)	(0.80)	(0.82)
NUMCHILD	1.086	-2.311	11.485	2.519	-0.924
	(0.56)	(0.74)	(2.19)	(0.93)	(0.30)
NUMEARN	-0.018	0.432	17.404	-2.620	4.682
	(0.01)	(0.14)	(2.02)	(0.73)	(1.02)
PRTPREV	0.734	-9.540	53.295	1.670	-5.543
	(0.05)	(0.52)	(1.80)	(0.11)	(0.30)
HINS	5.923	10.476	60.229	13.018	-5.132
	(0.37)	(1.26)	(1.87)	(0.91)	(0.45)
CLOSING	-4.572	-6.068	16.602		
	(0.74)	(1.05)	(1.56)		
RESID	-1.975	-8.891	51.333	-6.629	8.858
	(0.30)	(1.36)	(2.15)	(0.88)	(0.99)
STEUNION	0.412	0.364	1.912	0.606	0.452
	(0.74)	(0.75)	(1.87)	(1.10)	(0.67)
RTWORK	-5.824	-2.583	-25.081	-0.319	-13.436
	(0.82)	(0.32)	(2.27)	(0.03)	(1.43)
URATE	2.627	3.629	7.021	4.251	3.250
	(0.98)	(1.41)	(1.29)	(1.13)	(0.87)
AREARATE	-3.803	-3.581	-0.594	-3.826	-3.802
	(2.45)	(2.04)	(0.29)	(2.32)	(1.87)

INDCHG	-2.026	-1.148	-1.731	-0.985	-3.283
	(1.22)	(0.80)	(0.65)	(0.61)	(1.38)
OCCRATE	-0.128	-0.714	1.518	-1.055	1.365
	(0.08)	(0.54)	(0.66)	(0.71)	(0.86)
INDUNION	-0.282	-0.245	-0.099	-0.235	-0.587
	(0.65)	(0.66)	(0.19)	(0.71)	(0.95)
OCCUNION	0.451	0.250	1.447	0.513	0.543
	(1.98)	(1.00)	(2.53)	(1.45)	(1.36)
UIPROB	8.147	-1.321	-18.974	7.801	-6.614
	(0.40)	(0.05)	(0.64)	(0.37)	(0.19)
λ_0	41.821	12.218	88.291	23.833	-3.778
	(1.58)	(0.70)	(2.61)	(1.00)	(0.19)
λ_1	-5.090	-6.918	-125.804	-17.417	-31.941
	(0.11)	(0.28)	(1.78)	(0.50)	(1.32)
	(1)	(2)	(3)	(4)	(5)

Notes:

1. The switching model is of the form: $I^* = Z\gamma - \mu_2$; $W_j = X\beta_j + \mu_j$, where N=1 and j=1 if $Z\gamma \geq \mu_2$ and N=0 and j=0 if $Z\gamma < \mu_2$. Z contains all the regressors in X, plus dummy variables for states with mandatory or voluntary advance notice provisions. The estimates are obtained using a two-stage procedure, where the second-stage regression equations are: $W = X\beta_0 + X^*N(\beta_1-\beta_0) + \lambda_0\sigma_{02} + \lambda_1\sigma_{12} + \upsilon$, with $\lambda_0 = [\phi(.)/1-\Phi(.)]^*(1-N)$ and $\lambda_1 = [-\phi(.)/\Phi(.)]^*N$. $E(\upsilon) = 0$; $\phi(.)$ and $\Phi(.)$, respectively, are the p.d.f. and c.d.f. of the standard normal distribution evaluated at γZ.

2. Coefficients displayed are for the interaction terms X*N. Absolute values of asymptotic T statistics in parentheses. The sample is restricted to persons displaced between 1983 and 1985.

Table 6.3:
Switching Regression Estimates for Postdisplacement Joblessness
(Switching Criteria= Written or Unwritten Notice)

Regressor	All Workers	Males	Females	Closings	Layoffs
EXP	-0.154	-1.075	0.675	0.853	-0.594
	(0.24)	(1.38)	(0.59)	(0.84)	(0.56)
EXPSQ	6.0E-3	0.026	-0.012	-0.013	4.7E-3
	(0.42)	(1.48)	(0.47)	(0.53)	(0.19)
EDUC	-4.886	-4.584	-2.565	-3.112	-3.557
	(1.18)	(0.95)	(0.36)	(0.48)	(0.51)
EDUCSQ	0.213	0.213	0.113	0.139	0.166
	(1.33)	(1.16)	(0.41)	(0.57)	(0.64)
TENURE	-1.381	-0.725	-1.961	-1.814	-1.199
	(1.81)	(0.80)	(1.21)	(1.83)	(0.91)
TENSQ	0.038	0.015	0.062	0.040	0.084
	(1.41)	(0.47)	(0.85)	(1.23)	(1.65)
MARRIED	0.422	6.796	-9.483	7.857	-2.705
	(0.09)	(0.82)	(0.75)	(0.69)	(0.41)
HEAD	-4.324	-5.427	-11.362	-2.830	-5.828
	(1.05)	(0.75)	(0.96)	(0.47)	(0.88)
FEMALE	-0.192			7.500	-0.642
	(0.04)			(0.55)	(0.10)
BLACK	2.281	1.674	4.303	2.324	-0.942
	(0.46)	(0.28)	(0.49)	(0.29)	(0.12)
OTHRACE	-7.698	-22.660	11.981	-18.929	4.878
	(0.84)	(2.22)	(0.79)	(1.40)	(0.33)
NUMCHILD	-1.672	-4.327	0.184	-4.164	-1.022
	(1.04)	(1.80)	(0.07)	(0.96)	(0.52)
NUMEARN	-1.150	-0.157	-1.302	-3.900	0.906
	(0.63)	(0.06)	(0.39)	(1.12)	(0.33)
PRTPREV	7.106	7.240	6.605	12.728	9.874
	(0.86)	(0.63)	(0.56)	(0.84)	(0.93)
NINS	10.031	7.807	11.108	17.986	12.446
	(1.31)	(0.75)	(1.06)	(1.09)	(1.45)
CLOSING	2.585	1.004	2.216		
	(0.37)	(0.13)	(0.18)		
RESID	-4.536	-2.823	-4.918	-10.305	-2.749
	(1.19)	(0.62)	(0.74)	(1.20)	(0.53)
STEUNION	-0.158	-0.024	-0.327	-0.032	-0.409
	(0.56)	(0.07)	(0.66)	(0.08)	(0.95)
RTWORK	-0.022	0.280	-0.672	4.768	-6.588
	(0.01)	(0.06)	(0.09)	(0.80)	(1.07)
URATE	-1.294	0.787	-3.860	-0.395	-0.935
	(0.76)	(0.34)	(1.35)	(0.11)	(0.40)
AREARATE	-1.100	-1.352	-0.740	-1.924	-1.822
	(1.06)	(0.63)	(0.51)	(0.77)	(1.54)

INDCHG	-0.755	-0.719	-0.197	-0.148	0.081
	(0.87)	(0.84)	(0.92)	(0.10)	(0.06)
OCCRATE	0.692	0.648	0.505	0.488	1.657
	(0.89)	(0.74)	(0.35)	(0.37)	(1.60)
INDUNION	0.101	2.8E-3	0.032	0.317	0.110
	(0.42)	(0.01)	(0.06)	(0.97)	(0.30)
OCCUNION	-0.048	-0.084	0.084	-0.137	0.190
	(0.31)	(0.37)	(0.27)	(0.64)	(0.73)
UIPROB	4.172	3.541	-4.094	25.827	-4.136
	(0.31)	(0.17)	(0.19)	(0.88)	(0.22)
λ_0	-3.207	15.565	-24.759	30.916	-1.959
	(0.15)	(0.52)	(0.86)	(0.63)	(0.10)
λ_1	-9.227	9.010	-26.620	-11.112	-21.166
	(0.49)	(0.31)	(1.26)	(0.23)	(1.43)
	(1)	(2)	(3)	(4)	(5)

Notes:

1. The switching model is the same as in Table 6.2.

2. Coefficients displayed are for the interaction terms X*N. Absolute values of asymptotic T statistics in parentheses. The sample is restricted to persons displaced between 1983 and 1985.

Table 6.4:
Estimated Correlation Between Error Terms in Reemployment and Advance Notice Switching Equation

	Switching Variable	
	NOTICE	WRITTEN
Group		
All Workers:	6.9E-3	0.011
	(0.14)	(0.18)
Males:	-2.6E-3	0.012
	(0.04)	(0.15)
Females:	0.024	9.7E-3
	(0.29)	(0.09)
Closings:	-2.0E-3	0.014
	(0.03)	(0.15)
Layoffs:	0.017	3.0E-3
	(0.22)	(0.03)
	(1)	(2)

Notes:

1. Table shows estimated correlation between the switching regression and reemployment error terms (ρ) from FIML estimates of the bivariate probit regressions.

Table 6.5:
Switching Regression Estimates for Postdisplacement Wages
(Includes Correction for Reemployment Selection)

	Dependent Variable		
Regressor	NOTICE	WRITTEN	ADV3
EXP	-0.010	-0.021	-0.045
	(0.79)	(0.96)	(1.15)
EXPSQ	1.9E-4	4.7E-4	9.8E-4
	(0.68)	(1.08)	(1.19)
EDUC	-0.039	-0.041	-0.449
	(0.50)	(0.26)	(1.37)
EDUCSQ	6.1E-4	0.001	0.014
	(0.20)	(0.23)	(1.20)
TENURE	0.019	0.017	0.072
	(1.27)	(0.61)	(1.60)
TENSQ	-7.6E-4	-4.1E-4	-2.1E-3
	(1.42)	(0.66)	(2.06)
MARRIED	-0.132	-0.004	0.041
	(1.30)	(0.03)	(0.19)
HEAD	0.091	-0.060	-0.004
	(0.98)	(0.42)	(0.01)
FEMALE	-0.027	-0.084	0.394
	(0.27)	(0.48)	(1.25)
BLACK	-0.160	-0.166	0.092
	(1.70)	(1.19)	(0.29)
OTHRACE	0.200	0.210	-0.403
	(1.15)	(0.88)	(0.89)
NUMCHILD	0.040	0.030	0.081
	(1.24)	(0.79)	(1.08)
NUMEARN	0.071	-0.011	-0.153
	(0.98)	(0.10)	(0.73)
PRTPREV	-0.322	-0.086	-0.277
	(1.98)	(0.29)	(0.72)
HINS	0.005	0.147	0.274
	(0.03)	(0.52)	(0.56)
CLOSING	-0.060	0.173	0.079
	(0.45)	(1.49)	(0.24)
RESID	1.8E-4	0.082	0.242
	(0.00)	(0.65)	(0.83)
STEUNION	-0.010	-0.007	0.010
	(1.77)	(0.71)	(0.55)
RTWORK	-0.110	-0.055	0.019
	(1.37)	(0.41)	(0.08)
URATE	0.012	-0.010	-0.026
	(0.38)	(0.20)	(0.28)
AREARATE	0.019	0.033	0.028
	(0.95)	(1.10)	(0.77)

INDCHG	-0.007	0.008	0.049
	(0.42)	(0.26)	(1.10)
OCCRATE	-0.020	0.017	-0.020
	(1.36)	(0.62)	(0.41)
INDUNION	-0.001	0.003	0.004
	(0.27)	(0.40)	(0.46)
OCCUNION	-0.001	-0.005	0.007
	(0.40)	(1.24)	(0.64)
UIPROB	-0.420	-0.393	-0.731
	(1.55)	(0.97)	(1.21)
λ_W	0.081	-0.332	-0.618
	(0.37)	(0.97)	(0.97)
λ_0	-1.058	-1.209	-0.309
	(2.37)	(2.42)	(0.90)
λ_1	-0.790	-0.690	-0.479
	(2.29)	(0.82)	(0.62)
	(1)	(2)	(3)

Notes:

1. The switching model is of the form: $I^* = Z\gamma - \mu_2$; $W_j = X\beta_j + \lambda_W \gamma_j + \mu_j$, where N=1 and j=1 if $Z\gamma \geq \mu_2$ and N=0 and j=0 if $Z\gamma < \mu_2$. Z contains all the regressors in X, plus dummy variables for states with mandatory or voluntary advance notice provisions. The estimates are obtained using a two-stage procedure, where the second-stage regression equations are: $W = X\beta_0 + \lambda_W \gamma_0 + X{*}N(\beta_1 - \beta_0) + \lambda_W{*}N(\gamma_1 - \gamma_0) + \lambda_0\sigma_{02} + \lambda_1\sigma_{12} + \nu$, with $\lambda_0 = [\phi(.)/1-\Phi(.)]{*}(1-N)$ and $\lambda_1 = [-\phi(.)/\Phi(.)]{*}N$. λ_W is the inverse Mills ratio from a probit reemployment equation which includes all the regressors in Z, plus dummy variables indicating states with mandatory or voluntary advance notice provisions. $E(\nu) = 0$; $\phi(.)$ and $\Phi(.)$, respectively, are the p.d.f. and c.d.f. of the standard normal distribution evaluated at γZ.

2. Coefficients displayed are for the interaction terms X*N. Absolute values of asymptotic T statistics in parentheses. The sample is restricted to persons displaced between 1983 and 1985.

Table 6.6:
Switching Regression Estimates for Postdisplacement Wages
(No Correction for Reemployment Selection)

Dependent Variable

Regressor	NOTICE	WRITTEN	ADV3
EXP	-0.009	-0.023	-0.049
	(0.76)	(1.04)	(1.26)
EXPSQ	2.0E-4	4.5E-4	9.6E-4
	(0.70)	(1.03)	(1.17)
EDUC	-0.037	-0.032	-0.451
	(0.48)	(0.20)	(1.38)
EDUCSQ	5.1E-4	0.001	0.015
	(0.17)	(0.21)	(1.27)
TENURE	0.019	0.016	0.076
	(1.30)	(0.59)	(1.68)
TENSQ	-7.6E-4	-4.5E-4	-2.2E-3
	(1.43)	(0.71)	(2.27)
MARRIED	-0.118	-0.050	-0.024
	(1.24)	(0.42)	(0.11)
HEAD	0.074	0.012	0.150
	(0.92)	(0.10)	(0.47)
FEMALE	-0.030	-0.061	0.478
	(0.31)	(0.35)	(1.59)
BLACK	-0.159	-0.169	0.057
	(1.69)	(1.20)	(0.18)
OTHRACE	0.201	0.210	-0.312
	(1.16)	(0.88)	(0.71)
NUMCHILD	0.044	0.019	0.073
	(1.41)	(0.51)	(0.98)
NUMEARN	0.048	0.076	0.004
	(1.29)	(1.33)	(0.03)
PRTPREV	-0.321	-0.094	-0.249
	(1.98)	(0.32)	(0.65)
HINS	8.9E-4	0.161	0.314
	(0.01)	(0.57)	(0.64)
CLOSING	-0.064	0.187	0.106
	(0.47)	(1.64)	(0.33)
RESID	0.007	0.046	0.147
	(0.10)	(0.38)	(0.54)
STEUNION	-0.009	-0.008	0.008
	(1.76)	(0.78)	(0.41)
RTWORK	-0.116	-0.032	0.054
	(1.47)	(0.24)	(0.24)
URATE	0.010	-0.001	-0.008
	(0.31)	(0.03)	(0.09)
AREARATE	0.021	0.026	0.019
	(1.06)	(0.90)	(0.54)

INDCHG	-0.007	0.010	0.044
	(0.44)	(0.33)	(0.98)
OCCRATE	-0.019	0.016	-0.024
	(1.35)	(0.59)	(0.50)
INDUNION	-0.001	0.004	0.004
	(0.28)	(0.47)	(0.47)
OCCUNION	-0.001	-0.005	0.008
	(0.41)	(1.21)	(0.71)
UIPROB	-0.453	-0.252	-0.433
	(1.78)	(0.67)	(0.84)
λ_0	-1.059	-1.192	-0.289
	(2.38)	(2.39)	(0.85)
λ_1	-0.788	-0.680	-0.477
	(2.29)	(0.81)	(0.62)
	(1)	(2)	(3)

Notes:

1. The switching model is of the form: $I^* = Z\gamma - \mu_2$; $W_j = X_j\beta_j + \mu_j$, where N=1 and j=1 if $Z\gamma \geq \mu_2$ and N=0 and j=0 if $Z\gamma < \mu_2$. Z contains all the regressors in X, plus dummy variables for states with mandatory or voluntary advance notice provisions. The estimates are obtained using a two-stage procedure, where the second-stage regression equations are: $W = X\beta_0 + X^*N(\beta_1-\beta_0) + \lambda_0\sigma_{02} + \lambda_1\sigma_{12} + \upsilon$, with $\lambda_0 = [\phi(.)/1-\Phi(.)]^*(1-N)$ and $\lambda_1 = [-\phi(.)/\Phi(.)]^*N$. $E(\upsilon) = 0$; $\phi(.)$ and $\Phi(.)$, respectively, are the p.d.f. and c.d.f. of the standard normal distribution evaluated at γZ.

2. Coefficients displayed are for the interaction terms X*N. Absolute values of asymptotic T statistics in parentheses. The sample is restricted to persons displaced between 1983 and 1985.

Table 6.7:
Selection and Switching Coefficients for Switching Model with Correction for Reemployment Selection

Adv. Notice	Subsample				
Regressor	All Workers	Males	Females	Plant Closings	Layoffs
Notice					
λ_w	0.081	0.062	−0.083	0.090	0.173
	(0.37)	(0.19)	(0.26)	(0.32)	(0.47)
λ_0	−1.058	−0.519	−1.121	−1.865	−0.445
	(2.37)	(0.86)	(2.03)	(1.90)	(1.05)
λ_1	−0.790	−0.308	−0.211	0.388	−0.309
	(2.29)	(0.52)	(0.58)	(0.42)	(1.16)
Written					
λ_w	−0.332	−0.989	−0.061	−0.413	−0.029
	(0.97)	(1.62)	(0.11)	(0.80)	(0.06)
λ_0	−1.209	−0.067	0.727	0.079	0.010
	(2.42)	(0.19)	(1.10)	(0.17)	(0.02)
λ_1	−0.690	−0.190	−0.630	0.764	−0.623
	(0.82)	(0.40)	(0.48)	(1.24)	(1.36)
Adv3					
λ_w	−0.618	−1.337	0.126	−0.674	*
	(0.97)	(1.21)	(0.05)	(0.70)	
λ_0	−0.309	0.063	0.208	−0.270	0.595
	(0.90)	(0.18)	(0.42)	(0.63)	(1.90)
λ_1	−0.479	0.693	−3.679	−0.327	*
	(0.62)	(0.79)	(0.97)	(0.43)	

Notes:

1. The switching model is of the form discussed in Table 6.5.

2. Due to the small number of laidoff workers receiving written notice of greater than 2 months, λ_w and λ_1 were linear combinations of some of the covariates and so parameter estimates could not be obtained.

Table 6.8:
Selection and Switching Coefficients for Switching Model without Interactions between Advance Notice and Covariates

Adv. Notice Regressor	Subsample All Workers	 Males	 Females	 Plant Closings	 Layoffs
Notice					
β_n	-1.446	-0.765	-0.857	-0.882	-0.608
	(3.36)	(1.13)	(1.86)	(0.84)	(1.69)
λ_0	-0.851	-0.390	-0.582	-0.460	-0.480
	(3.15)	(0.93)	(2.00)	(0.75)	(1.89)
λ_1	-0.959	-0.601	-0.438	-0.676	-0.275
	(3.41)	(1.40)	(1.43)	(0.97)	(1.21)
Written					
β_n	-1.092	-0.300	0.754	0.289	-0.364
	(2.23)	(0.84)	(1.12)	(0.64)	(0.90)
λ_0	-1.279	-0.169	0.556	0.190	-0.320
	(2.68)	(0.51)	(0.89)	(0.44)	(0.85)
λ_1	-0.458	-0.189	0.406	0.104	-0.109
	(1.92)	(1.01)	(1.22)	(0.45)	(0.50)
Adv3					
β_n	0.263	0.239	0.406	0.443	0.107
	(0.85)	(0.63)	(0.93)	(1.19)	(0.22)
λ_0	-0.167	0.174	0.202	-0.248	0.613
	(0.50)	(0.50)	(0.42)	(0.59)	(2.03)
λ_1	0.090	0.086	0.069	0.192	-0.035
	(0.59)	(0.45)	(0.31)	(1.01)	(0.14)

Notes:

1. The switching model is of the form: $I^* = Z\gamma - \mu_2$; $W_j = X\,\beta + N\,\beta_j + \mu_j$, where N=1 and j=1 if $Z\gamma \geq \mu_2$ and N=0 and j=0 if $Z\gamma < \mu_2$. Z contains all the regressors in X, plus dummy variables for states with mandatory or voluntary advance notice provisions. The estimates are obtained using a two-stage procedure, where the second-stage regression equations are: $W = X\,\beta + \lambda_w\,\gamma + N\,\beta_n + \lambda_0\sigma_{02} + \lambda_1\sigma_{12} + v$, with $\lambda_0 = [\phi(.)/1-\Phi(.)]*(1-N)$, $\lambda_1 = [-\phi(.)/\Phi(.)]*N$ and $\beta_n = (\beta_1-\beta_0)$. λ_w is the inverse Mills ratio from a probit reemployment equation which includes all the regressors in Z, plus dummy variables indicating states with mandatory or voluntary advance notice provisions. $E(v) = 0$; $\phi(.)$ and $\Phi(.)$, respectively, are the p.d.f. and c.d.f. of the standard normal distribution evaluated at γZ.

Chapter 7:

Conclusions and Policy Implications

This report analyzes the effects of advance notification on postdisplacement joblessness and wage changes using a new data source which differentiates between written notice and less formal types of early warning. This distinction is crucial. Research examining the impact of informal notification, which includes both verbal announcements and ill-defined "expectations" of impending displacements, has limited relevance for the current policy debate on the efficacy of legislation mandating specific written advance notice. For instance, some workers receiving general information on the possibility of future layoffs (without precise knowledge about when or who will be terminated) immediately begin searching for new jobs and may become reemployed relatively quickly once displacement occurs. This provides little information, however, about the consequences of mandatory written notice.

Empirically, the effects of formal and informal notice are very different. Workers expecting terminations are much more likely to obtain new positions without intervening nonemployment than individuals surprised by their loss of jobs. Failing this, they have somewhat shorter durations of nonemployment. In contrast, written advance notice is associated with smaller increases in the probability of time out-of-work and with no reduction in average joblessness. Conversely, persons receiving more than 2 months formal notice of impending displacements have considerably higher survey date earnings than their counterparts but there is no evidence of wage gains for either informally notified individuals or for those receiving written notice shortly before the loss of jobs.

The reduced form regression estimates predict increased joblessness for persons notified in writing and, among this group, lower wages for individuals losing jobs as the result of partial layoffs. Absent any theoretical reason why prior notice should lengthen durations or decrease wages, these results suggest misspecification of the estimated model, quite possibly because of the failure to account for the endogeneity of advance notice.

A switching regression model (with endogenous switching) was estimated in an attempt to solve the endogeneity problem. Since the switching framework assumes the dependent variable is linear in the covariates, we first examined whether this assumption imposes undesirable restrictions when examining postdisplacement joblessness, given the presence of right censored observation and truncation of the dependent variable at zero. The determinants of nonemployment durations were found to be robustly estimated across a wide variety of models. If anything, those which assume the dependent variable is linear in the covariates (e.g. OLS, tobit) performed better than those where the log of joblessness is linearly related to the regressors (log-linear, accelerated failure time).

Although the switching regressions fail to fully account for the endogeneity of notice, they do indicate important differences across population subgroups. Prior notification leads to relatively large reductions in nonemployment durations for three categories of special concern — married persons, household heads, and displaced workers located in regions of high unemployment — as well as for highly educated individuals. Intergroup differences are less pronounced for survey date earnings but endogeneity bias appears of greater concern. Unfortunately, given the inability of the switching model to fully correct for endogeneity bias, we feel safe only in concluding that the reduced form estimates provide an upper bound on the potential wage benefits of of prior notification.

7.1 Implications for Policy

With the passage of the Worker Adjustment and Retraining Notification Act, the United States has joined virtually all other industrialized countries in regulating plant shutdowns and mass layoffs. Given the short period of time since the law has taken effect, it is too early to assess the degree to which it has changed employer behavior or assisted displaced workers. The results of this study, however, suggest that the provision of at least 2 months written advance notice is likely to lead to modest benefits for displaced workers. In particular, these persons are expected to be somewhat more likely to avoid joblessness and to have slightly higher reemployment earnings. Nonemployment durations are also substantially reduced for a number of groups for whom nonemployment is of special concern.

The benefits of advance notice must be weighed against any costs to employers. Unfortunately, reliable research on the latter is virtually nonexistent. Despite predictions that customers may disappear, access to credit markets will be impaired, productivity reduced, or absenteeism increased, the limited information which is available provides little evidence of any serious negative consequences for business. In contrast, the data show that productivity often rises following the notice and, in some cases, notification results in actions which save rather than destroy the plant

(Berenbeim, 1986; Sutton, 1987). Some European countries provide examples showing that extremely restrictive labor legislation can inhibit employment growth; however, the Japanese experience indicates that substantial advance notice and economic prosperity are not incompatible.

On balance, the existing research, including that presented in this report, lends provisional support to the advance notice legislation passed in 1988. Beyond any economic consequences, workers favor mandatory early notification because they feel it to be fair, while the corporate sector expresses concern that it represents a first step towards more onerous government interference. Debate over the proper role of the government role is likely to continue. Nonetheless, employment security appears to have joined child labor, occupational safety, and equal opportunity, as an area where federal regulation has become an accepted fact. It is therefore pertinent to ask whether the existing mandatory notice law provides adequate protection to workers while minimizing costs to firms.

According to the current statute, most employers are required to provide 60 days advance notice of layoffs or plant closings. Exemptions are provided for: small employers; companies terminating less than 50 workers or those releasing fewer than 500 workers and less than a third of their workforce; firms effected by "unforeseeable" business developments, strikes, or lockouts and those actively seeking new capital or business or closing plants as the result of sale of the business or consolidation within the local area; enterprises offering displaced workers new positions.

These exemptions substantially reduce the number of workers who will receive prior notice. The vague nature of some of the exclusions may also encourage circumvention of the spirit of the law and undermine the attempts of many business groups to increase the extent of advance notice provided. This argues for broader coverage and more clearly delineated exemptions.

For example, the small business exclusion should probably be limited to smaller firms (say those employing fewer than 25 workers) and the threshold at which companies must provide advance notice of partial plant closings reduced from 500 to 100 persons. The exclusion for closures due to "unforeseeable" business developments should be deleted because it is hopelessly difficult to interpret. Similarly, since one of the express purposes of the law is to make businesses consider the social costs of worker dislocation which result from corporate restructuring, it makes no sense to exempt displacements occurring because of a business sale or consolidation. Finally, the exemption for companies seeking new business or capital should be specified in greater detail or eliminated. To avoid raising costs to businesses, this broader legislative coverage could be combined with a number of mechanisms to reward firms meeting mandated standards. For instance, more favorable unemployment insurance treatment could be granted to complying companies, in combination with larger increases in UI premiums for those failing to do so.[1]

7.2 Directions for Future Research

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[1] This would require a more fully experience rated UI system than presently exists (around half of all firms currently pay the maximum premium and so bear no extra insurance costs for additional layoffs). Most economists believe that more complete experience rating is desirable because it would reduce current government subsidies to firms using large amounts of temporary or seasonal labor.

The findings discussed in previous chapters suggest a number of directions for subsequent research. First, more sophisticated attempts need to be made to control for the endogeneity of advance notice. In particular, future work should consider whether jobs offering advance notice are rationed in such a way that enterprise characteristics (rather than individual traits) are of primary importance in determining notification status. The findings of such a study would also be likely to lead to a more general understanding of how the employee/job matching process works.

Second, the techniques by which workers become informed about future displacements needs to be explicitly modeled. Until this is done, it is be unclear to what extent advance notice actually provides workers with new information. This is particularly important given the evidence that workers expecting terminations in the absence of advance notice avoid some of the adjustment problems faced by their formally notified counterparts.

Third, greater attention should be paid to demographic group variations in the effects of prior notification. As shown in the analysis of nonemployment durations, these differences are far larger than the average effect. Similarly, arguments by proponents and opponents of mandatory advance notice should focus on the impact for groups over which there is particular concern.

Fourth, further investigation is needed into how the effects of early warning vary with the timing of its receipt. The preliminary work presented in this report suggests that benefits are greatest for individuals formally notified more than 2 months before displacements. An important next step would involve examining the impact of still lengthier prenotification. This would be especially useful since the costs of mandated early warning are likely to increase considerably when the notification period exceeds 60 days.

Finally, future Displaced Worker Supplements to the Current Population Survey should be modified in several ways to increase the information available to researchers. Subsequent surveys should distinguish between workers expecting job displacements in the absence of prenotification and those receiving specific verbal announcements of impending job losses. In addition, the categorical information on the duration of written advance notice should be replaced by a question allowing a continuous range of responses. More generally, the DWS would become much more useful with the incorporation of 2 changes. First, questions on prior employment conditions should be asked of some proportion of nondisplaced workers. This would allow researchers to construct an appropriate control group for the first time. Second, the survey should be made partially longitudinal by resurveying a proportion of displaced (and nondisplaced) individuals 2 years in the future. This would make it possible to obtain considerable additional.information on the time profile of postdisplacement changes in working and living conditions.

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